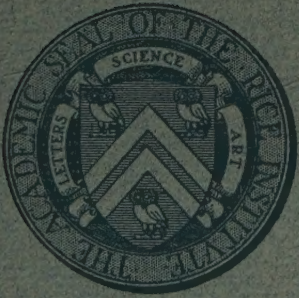
THE
RICE INSTITUTE PAMPHLET

Vol. XV October, 1928 No. 4



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A university of liberal and technical learning founded by William Marsh Rice in the City of Houston, Texas, and dedicated by him to the advancement of Letters, Science, and Art

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THE METHODS OF RESEARCH[1]

I

BACK TO ARISTOTLE

SOME fifteen years or more ago I wrote to T. W. Richards at Harvard commenting on something that he had either done or left undone—it does not matter now which. He answered, almost by return mail: "There are nine and sixty ways of writing tribal lays, and every single one of them is right." That ended the discussion for the time being; but I have been thinking over the matter ever since. The great American public is firmly convinced that there can only be fifty-seven varieties of pickles, and pickle-making is a much more highly developed industry than that of writing tribal lays. We only need seven crystal systems in which to classify the innumerable crystal forms which are known. Robert Louis Stevenson says that there are only three ways of writing short stories, and some Frenchman, whose name I do not recall, has said that there are only three plots. Things are much simpler than one usually assumes. I am defending the thesis that there are only two methods of research and that one of these is bad. Unfortunately, most of us preach the false doctrine to our students.

The first of the two general methods of research is the deductive or Aristotelian method in which one first gets one's working hypothesis and then accumulates data to test it. The second general method is the inductive or Baconian method in which one accumulates data until the general theory underlying them becomes obvious.

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[1] A course of three lectures delivered by Professor Wilder D. Bancroft, Ph.D., D.Sc., of Cornell University, in the Chemistry Lecture Hall of the Rice Institute, April 9, 10, and 11, 1928.

Aristotle[1] says that "we acquire knowledge either by Induction or by Demonstration: and Demonstration is from universals, but Induction from particulars. It is impossible to have universal theoretical propositions except by Induction: and we cannot make inductions without sensations; for sensations have to do with particulars."

Bacon[2] is a bit more definite.

"There are and can be but two ways of investigating and discovering Truth. The one leaps from the senses and particulars to the most general axioms, and from these as first principles, and their unshaken truth, judges on and discovers medial axioms: and this way is in vogue. The other raises axioms from the senses and particulars, by ascending steadily, step by step, so that at last the most general may be reached; and this way is the true one but untried."

"Our method of discovering the Sciences is one which leaves not much to acumen and strength of wit, but nearly levels all wits and intellectuals."

"We must say of ourselves what one said jesting—especially as it hits the matter so well—that water drinkers and wine drinkers cannot think alike. For all men, hitherto, ancient or modern, have in learning drunk but a crude liquor like water, which has either flowed of its own accord from the Intellect, or has been drawn up as by wheels of a well, by means of Logic. But we drink and pledge others with a liquor made of many well-ripened and mature grapes, gathered and plucked from particular clusters; then pressed in the wine-press; and lastly cleansed and clarified in a vessel. And so no marvel that we and others do not agree."

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[1] Whewell: "Philosophy of Discovery," 19 (1860).

[2] "Novum Organum" (translated by G. W. Kitchin), 14, 32, 103 (1855).

Bacon's presentation of his case would certainly be ill-advised, and perhaps illegal, in these United States today. It is quite clear, however, that he believes that the general relations should be so obvious from the experiments, that even those of little wit will recognize the truth and necessity of the conclusions. Consequently, a general truth cannot have been reached by the Baconian method if it strikes a large number of people interested in the subject as being absurd and foolish. E. O. von Lippmann[1] says that if a conclusion is not obvious to other scientific men when stated it cannot have been reached by the Baconian method. The converse is of course not true, that a discovery must have been made by the Baconian method if it is accepted generally in a very short time. The way in which the discovery was presented to the world may have been especially good.

The objection is sometimes raised that one cannot make a working hypothesis without some facts on which to base it. This is true enough; but is not really relevant. One might discuss at length and unprofitably which came first, the hen or the egg. One must use common-sense even when doing research work. Of course one makes use of all the knowledge one has in formulating a working hypothesis. If there are no facts available, some must be got; but the difference between the two types of men is that one set gets its working hypothesis early in the game and the other late. It is probable that van't Hoff would never have deduced the theory of solutions from his inner consciousness; but it is surprising how few facts were necessary.

I do not sympathize at all with the point of view expressed by Henry Fairfield Osborn.[1] Fifty years as a teacher have afforded an original retrospect and prospect of the art of education, an art which has an unchanging element in the vicissitudes of our environment which we call civilization. Throughout this long half-century period I have been consistent with my oft-repeated advice to my students, namely, to think a subject out for oneself and then to read what others have thought about it. I have myself given an immense amount of personal thought to the methods of intellectual education, and I am free to confess that I have depended very little on reading of the works even of the great masters and innovators to whom I refer from time to time in this volume. My rule with myself and with my students has been, first, to try to master a subject or to thoroughly understand it; second to try to add something of my own to this subject—that is, to produce or create something new."

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[1] "Zur Geschichte der Naturwissenschaften," 505 (1906).

I have known men who made a point of not looking up what had been done before starting on a problem because they were afraid of getting into a rut and seeing things as their predecessors saw them. There is this danger, especially if one stands a little in awe of the printed page; but the remedy is to learn to read critically. The other way one is sure to waste a large amount of time in repeating the work of others. I have found it helpful to see what the flaws were in the reasoning and to try to think out an explanation which can be distorted into each one of the suggested hypotheses. What one should then do is to supplement the other man's work, with the smallest number of experiments needed to carry conviction. There is nothing that I delight in more than a problem in which ninety per cent of the work has already been done.

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[1] Natural History, *27*, 309 (1927).

The difficulty with classifying research men as Baconians and Aristotelians is that these names have other connotations which confuse the issue. While the Greek method of attack was sound in principle, the Greeks were distinctly bad about doing the experimental work necessary to verify their working hypotheses. On the other hand Bacon did lay great emphasis on the importance of making experiments and one is apt to find that this is what the other man is talking about instead of about Bacon's research methods.

The words inductive and deductive would be useful if people agreed as to what they mean. Most of us would class Bacon as the exponent of inductive reasoning and Aristotle as the exponent of deductive reasoning; but Mellor[1] reverses this and says that the deductive method was favored by Francis Bacon and the inductive method by Isaac Newton. Mellor goes so far as to say that "the method of Aristotle was rediscovered and restated by Francis Bacon in his Novum Organum," a statement which would have surprised Bacon a good deal if he could have heard it.

It seems wiser therefore to invent entirely new terms which shall have only the connotations which I choose to give them. I am going to say that those who accumulate facts are accumulators and those who start early with working hypotheses are guessers. Even this apparently simple statement needs some explanation. I am using the word guess as it is used in New England and not as it is used in Central New York. I do not mean the result of pure chance, such as flipping a coin or deciding whether to finesse on the right or the left when the bidding has shown nothing. I mean by guessing making the shrewdest judgment one can from facts which are not entirely adequate. Guess is the

[1] "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1,* 17 (1922).

word used by Newton in the same sense, so it has been in good use for several centuries.

It is still rather the fashion for people to have slogans and the slogan of the accumulators is: "First get your facts." I have heard that said to unfortunate students time and again. The slogan of the guessers is: "First get your working hypotheses." I use the plural advisedly because it is always better to have several working hypotheses if possible.

People do and justify what interests them. Accumulators must develop manipulative skill, and experiment becomes to them only too often an end in itself. Guessers are interested primarily in theory and do not care much about experiments *per se*. Consequently, they are often accused of not being able to do experiments. As a rule, that does not worry them. Sir Humphry Davy[1] said: "I thank God that I was not made a dexterous manipulator, for the most important of my discoveries have been suggested to me by failures." Sir William Hamilton,[2] the inventor of quaternions, said: "In physical sciences the discovery of new facts is open to every blockhead with patience, manual dexterity, and acute senses." Perhaps it would be fairer to quote now from Lord Kelvin.[3] "Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. Yet nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient, long-continued labour in the minute sifting of numerical results."

The distinction between the accumulator and the guesser has been put very tactfully by A. V. Hill when speaking of Martha and Mary in Science. "Martha represents the scientist always busy in his laboratory collecting invaluable data. Mary takes the broad view, reflects and discusses, and listens to discussion. Science needs them both."

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[1] Mellor: "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1*, 569 (1922).

[2] "Discussions on Philosophy and Literature," 239 (1852).

[3] Gregory: "Discovery," 60 (1923).

Bosanquet[1] has said the same thing in truly orthodox language. "In the gradual discrimination of the inquiries initiated confusedly by the omnivorous appetite for knowledge, two classes of truth come to be plainly distinguished—statements of particular facts, and statements of general connections. These constitute the branches of knowledge which fulfil less or more completely the ideal of science."

It will simplify discussion if I give a partial classification of some scientific men. Among the guessers, I put Galileo, Harvey, Kepler, Newton, Huygens, Young, Lavoisier, Dalton, Davy, Avogadro, Gauss, Franklin, Faraday, Joule, Darwin, Mendel, Pasteur, Lister, Helmholtz, Gibbs, van't Hoff, Arrhenius, J. J. Thomson, Rutherford, Einstein, and Bohr. Among the accumulators I place Tyco Brahe, Leeuwenhoek, Priestley, Stas, Becquerel, Kohlrausch, Newcomb, Raoult, Röntgen, Dewar, Fabre, Richards, Sabatier, Mme. Curie, and Millikan.

People will not be unanimous over this grouping, so it will be well to say a few words about some of the men, so as to justify the classification.

There can be no question about Galileo. His conclusion as to the motion of the earth was disputed by nearly everybody at the time, and even his experiment on falling bodies at the tower of Pisa did not convince many of his audience, conclusive though it may now seem to us.[2]

"Members of the University of Pisa, and other onlookers, are assembled in the space at the foot of the wonderful leaning tower of white marble in that city in the year 1591. A young professor climbs the spiral staircase until he reaches the gallery surmounting the seventh tier of arches. The people below watch him as he balances two balls on the edge of the gallery, one weighing a hundred times more than the other. The balls are released at the same instant and are seen to keep together as they fall through the air until they are heard to strike the ground at the same moment. Nature has spoken with no uncertain sound, and has given an immediate answer to a question debated for two thousand years. 'This meddlesome man Galileo must be suppressed,' murmured the University fathers as they left the square. 'Does he think that by showing us that a heavy and a light ball fall to the ground together he can shake our belief in the philosophy which teaches that a ball weighing one hundred pounds will fall one hundred times as fast as one weighing a single pound. Such disregard of authority is dangerous and we will see that it goes no further.' So they returned to their books to explain away the evidence of their senses; and they hated the man who had disturbed their philosophic serenity."

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[1] "Science and Philosophy," 23 (1927).

[2] Gregory: "Discovery," 2 (1923).

In connection with this I wish to quote the delicious argument[1] from James Branch Cabell's "Something about Eve." Most of us do much the same thing to some extent.

"Ah, but I must tell you," said Tenjo, seeming yet more troubled, "that the man who looks into that mirror straightway finds himself transformed into two stones. For this reason it is hidden away in Peter's Tomb, and it is kept veiled, and of course no man has ever dared go near it."

"How, then, did this mirror ever manage to change anybody into two stones if nobody ever dared go near it?"

"Why, but the mirror was compelled to change them into two stones because that was the law. It was not at all the

[1] p. 116.

mirror's fault. . . . And the people kept away from the mirror because they knew about this law. Surely, that too was natural?"

"In a way, yes. But how could they be certain about this law?"

"How could they help it, how could anybody be ignorant of one of our very oldest and most famous laws, which comes down to us, indeed, from sources so august and venerable that they antedate all history?"

"Why, then, who enacted this law?"

"How should I know, when, as I was just telling you, this law is older than any recorded history? . . ."

"Yet, do you but answer me this very simple question? What if some intelligent, unsuperstitious person were to look in this mirror,—and were to come back not changed into stone, and not hurt in any way,—would that not prove to you the insanity of this law?"

"Of course it would not! That would only prove the man was a liar. The plain fact of his not being changed into two stones would be legal proof in any of our courts or in any law-respecting place anywhere that he had not ever looked into the Mirror of the two Truths."

Gregory[1] considers that "Harvey's work was an excellent example of the application of the inductive method of study laid down by Francis Bacon as the essential principle of scientific principle; but Harvey did not begin to teach the circulation of the blood until 1619, and as Bacon died seven years later he may be forgiven the omission of any reference to it in his writings, though he must have known of it."

Gregory is confusing, as so many people do, Bacon's emphasis on the importance of experiment and Bacon's method of inductive reasoning. We know that Harvey would have objected to his work being classed as an excellent example of Bacon's inductive method, because Harvey himself said of Bacon that "he writes philosophy like a Lord Chancellor."

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[1] "Discovery," 139 (1923).

In Harvey's own account of his work,[1] he gives "a description of the heart as seen in a living animal when the chest has been laid open and the pericardium removed. Three circumstances are noted:—

(*a*) The heart becomes erect, strikes the chest, and gives a beat;
(*b*) It is constricted in every direction;
(*c*) Grasped by the hand, it is felt to become harder during the contraction.

"From these circumstances it is inferred:—

(1) That the action of the heart is essentially of the same nature as that of voluntary muscles, which become hard and condensed when they act;
(2) That, as the effect of this, the capacity of the cavities is diminished, and the blood is expelled;
(3) That the intrinsic motion of the heart is the systole, and not the diastole, as previously imagined.

"The motions of the arteries are next shown to be dependent upon the action of the heart, because the arteries are distended by the wave of blood that is thrown into them, being filled like sacs or bladders, and not expanding like bellows. These conclusions are confirmed by the jerking way in which blood flows from a cut artery.

"In the heart itself two distinct motions are observed—first of the auricles and then of the ventricles. These alternate contractions and dilatations can have but one result, namely, to force the blood from the auricle to the ventricle, and from the ventricle, on the right side, by the pulmonary artery to the lungs, and on the left side by the aorta to the system."

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[1] McRae: "Fathers of Biology," 88 (1910).

These considerations suggest to the mind of Harvey the idea of the circulation. "I begin to think," he says, "whether there might not be a motion, as it were, in a circle." This is next established by proving the three following propositions:—

(1) The blood is incessantly transmitted by the action of the heart from the vena cava to the arteries in such quantity that it cannot be supplied from the ingesta, and in such wise that the whole mass must very quickly pass through the organ;

(2) The blood, under the influence of the arterial pulse, enters, and is impelled in a continuous, equable, and incessant stream through every part and member of the body, in much larger quantity than were sufficient for nutrition, or than the whole mass of fluids could supply;

(3) The veins in like manner return this blood incessantly to the heart from all parts and members of the body.

While there is no time-scale to these paragraphs, it is quite evident that Harvey started generalizing as soon as possible. He lectured on the subject for nine years before he published his views, which were received by most of his contemporaries with doubt and with scorn. Gregory[1] himself admits that Harvey did not and could not make out the whole course of the blood.

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[1] "Discovery," 138 (1923).

"Harvey established absolutely the fact of the circulation of the blood, and the fact that the muscular action of the heart causes this movement. But he was unable, from his want of a microscope, to indicate the precise path along which the blood travels from the terminal arteries to the commencing veins. The large artery from the heart gives off branches to various parts of the body, and these branch off again into small arteries in different organs. Similarly, small veins carrying blood back to the heart unite to form large veins. How the blood passed from the small arteries to the small veins could only be conjectured by Harvey, and was not discovered until three years after his death. He concluded that the blood passes from the arteries to the veins mainly by percolation, as water, to use his own illustration, percolates the earth and produces springs and rivulets. No microscope in his time was powerful enough to enable him to see the meshwork of very minute tubes—the capillaries—which can now be observed easily."

Gregory[1] says in regard to Kepler: "In the process of discovery of the three fundamental laws known by his name, Kepler was led to make many fantastic hypotheses. But all through he was guided by the principle that God who made the world had established fixed laws throughout his works, laws that are often so definite as to be capable of expression in exact numerical terms. In accordance with these views he sought for numerical relations in the disposition of the planets and their arrangement, in respect to their number, their times of revolution, and their distances from one another. Each hypothesis he made, however fanciful, he tried by a vigorous test whenever possible, and, as soon as he found that the facts were not in accordance therewith, he abandoned it, and without hesitation proceeded to try others, which he submitted to the same severe ordeal, to share perhaps the same fate. He says, 'After many failures, I was comforted by observing that the motions in every case seemed to be connected with the distances; and that when there was a great gap between the orbits there was the same between the motions.' He was at length led to the discovery of his well-known 'Harmonic' law that the squares of the periodic times of revolution of the planets are as the cubes of their mean distances from the Sun."

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[1] "Discovery," 142 (1923).

Some people have claimed that Newton was not a guesser because he said that he did not make hypotheses; but all that this means is that he did not make speculative hypotheses which could not be tested. In a talk before the Research Club at Cornell, H. A. Lorentz said that Newton's questions were really hypotheses. E. O. von Lippmann[1] is equally definite. "While Newton said 'I do not make hypotheses,' he made them nevertheless in almost all his work. Nothing else could be expected, because, without hypotheses, one cannot formulate the laws which experiment confirms by questioning nature, in other words no induction. Not to have seen this is one of the greatest errors with which one must reproach Bacon."

Woodruff[2] says that "the particular form which the Newtonian method takes in science is to devise provisional generalisations called hypotheses or *working hypotheses* to explain facts and phenomena. The appeal is then made to observation and experiment in order to test the validity of the proposed generalisation."

Mellor[3] quotes Newton himself as saying that "no great discovery was ever made without a bold guess."

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[1] "Zur Geschichte der Naturwissenschaften," 423 (1906).

[2] "Development of the Sciences," 134 (1923).

[3] "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1,* 18 (1922).

The opposition of Newton was sufficient to keep the undulatory theory of light in the background for many years, so that it is quite evident that Huygens did not develop his theory by following the Baconian method. He was a guesser.[1] "Although Robert Hooke in 1668 and Ignace Pardies in 1672 had adopted a vibratory hypothesis of light, the conception was a mere floating possibility until Huygens provided it with a sure foundation. His powerful scientific imagination enables him to realize that all the points of a wave-front originate partial waves, the aggregate effect of which is to reconstitute the primary disturbances at the subsequent stages of its advance, thus accomplishing its propagation; so that each primary undulation is the envelope of an indefinite number of secondary undulations. This resolution of the original wave is the well-known 'Principle of Huygens,' and by its means he was enabled to prove the fundamental laws of optics, and to assign the correct construction for the direction of the extraordinary ray in uniaxial crystals. These investigations, together with his discovery of the 'wonderful phenomenon' of polarisation, are recorded in his 'Traité de la lumière,' published at Leiden in 1690, but composed in 1678."

The work done by the "powerful scientific imagination" is of course what Newton meant by a bold guess. According to Gregory[2] the conclusive test of the undulatory theory was not given until 1850, nearly two centuries after the theory was developed.

"Newton supported with the weight of his great authority the theory that light is due to the emission of minute particles, at a high velocity by a luminous body. When these minute particles (corpuscles) impinge upon the retina of the eye, they produce the sensation of light. According to this view, light ought to travel more quickly in water than in air. Another theory, put forward by Huygens about Newton's time, and developed later, is that light is due to vibration in an imaginary medium called the ether, believed to prevail throughout all space. Luminous bodies set the ether in vibration, and when these undulations reach the eye they give us the sensation of light. On this view, light should travel more slowly in a substance like water than it does in air.

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[1] "Encyclopædia Britannica," *14,* 21 (1910).

[2] "Discovery," 158 (1923).

"To test the two theories, therefore, an experiment was required by which the relative velocities of light in air and water could be determined. By Newton's emission theory the velocity should be greater in water than in air; while it should be less according to Huygens' undulatory theory. Not until the middle of the nineteenth century was a means found of determining experimentally the velocities of light in water and air in a laboratory. The crucial experiment which would decide which theory was true was performed by a French physicist, Jean Léon Foucault, in 1850; and it showed that light, which travels at the rate of about 186,000 miles a second in air, travels throughout water at about three-quarters that velocity. The result of this experiment disposed finally of the emission theory, and re-established Huygens' theory that light is due to very rapid vibrations in a hypothetical ether pervading the universe."

The very next paragraph in Gregory's book establishes the case for Young as a guesser. "Fifty years before the crucial determination was made of the velocities of light in water and air, a genius of the first magnitude—Dr. Thomas Young—had shown that red light is produced by nearly 32,000 ether-waves to the inch, and that the number of such waves in a given length increases progressively in passing from red to violet, until at this end of the colour scale there are about 60,000 undulations to the inch. Young also proved that certain optical effects could be explained only by the principle of interference of ether-waves with one another; but his researches and interpretations, involving, as they did, the existence of an imponderable ether which, to use his words 'pervades the substance of all material bodies, with little or no resistance, as freely, perhaps, as the wind passes through a grove of trees,' met with ridicule from leaders in the literary world, and were not given serious attention by his scientific contemporaries. When, in 1815, a French investigator, Augustin Jean Fresnel, began experimental work in optics, and was also led to the discovery of interference in light, he knew nothing of the previous work done by Young in the same direction thirteen years earlier. The work of these two investigators revived the undulatory theory and opened a question which may be said not to have been settled decisively until Foucault's crucial experiment had been made."

Lavoisier[1] overthrew the phlogiston theory in the only way that a theory can be overthrown, by introducing a better theory, the oxygen theory. His attack on phlogiston was very severe. "Chemists have turned phlogiston into a vague principle which consequently adapts itself to all the explanations for which it may be required. Sometimes this principle has weight and sometimes it has not; sometimes it is free fire and sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, sometimes these are impervious to it: it explains both causticity and non-causticity; transparency and opacity, colours and their absence. It is a veritable Proteus, changing its form at each instant."

Priestley and Scheele, the two discoverers of oxygen, were phlogistonists.[1] "Priestley named the gas which would not burn, but which removed the combustible property from combustibles, and which was so far from phlogiston that it was purer than common air—'*dephlogisticated air.*' The name showed that he was even yet a phlogistonist, and in fact he remained so to the end of his life, although his own experiments had destroyed the theory. . . . Scheele argued that if the effect of combustion between phlogiston and air is to cause contraction, the remaining air ought to be heavier. On weighing it he found it to be *slightly lighter*. Hence, he inferred that part of the air must have disappeared, and that common air must consist of two gases, one of which has the property of uniting with phlogiston. In order to find out what had become of the air which had disappeared, he heated many metals, phosphorus, and other substances in air, and found that they behave in the same way as the iron and the sulphide. His inference was that the compound formed by the union of phlogiston with one of the constituents of air is the heat, or the fire, which escaped through the glass. He fancied he had proved the decomposition of the substance heat into *phlogiston and fire-air!* He decomposed heat chemically, he thought, by heating manganese dioxide with sulphuric acid, and again by heating the red calx of mercury. The calx combined with the phlogiston of the heat, and the other constituent of heat was fire-air, which he collected. This was oxygen."

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[1] Campbell Brown: "A History of Chemistry," 248 (1920).

Although Lavoisier was not the discoverer of oxygen, it was he who developed the oxygen theory of combustion. As these quotations have shown, the oxygen theory was far from obvious and met with considerable opposition. Consequently we must class Lavoisier as a guesser and not as a Baconian.

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[1] Ibid., 268.

A quotation from Perrin[1] kills two birds, clearing up the case of Dalton, the chemist, and that of Boltzmann, the mathematical physicist. "To divine in this way the existence and properties of objects that still lie outside our ken, *to explain* the complications of the visible in terms of invisible simplicity, is the function of the intuitive intelligence which, thanks to men such as Dalton and Boltzmann, have given us the doctrine of atoms." Perrin does not lay special emphasis on the word "intuitive;" but it is the important one for us. Laue's discovery of the action of X-rays on crystals was certainly a triumph of intuitive reasoning, a bold guess, because there were absolutely no experiments along this line. Laue was searching for a phenomenon which nobody had ever seen; but which had been deduced from the fact that X-rays were short waves. No one can dispute the classification of Benjamin Franklin as a guesser.

In 1811 Avogadro published what is now known as his law, that equal volumes of gases at the same temperature and pressure contain equal numbers of molecules. This law is nowadays given to beginners in chemistry very early in the course; but it was neither understood nor appreciated by chemists until after the reform of Cannizzaro in 1858. We must therefore classify Avogadro as a guesser since his generalization does not conform in any respect to the criteria laid down by Bacon.

We are not in the habit of looking upon mathematicians as guessers; but that is apparently because most of us do not happen to know how the mind of a mathematician works. Huygens published his theorems on centrifugal force without proof. Gauss said: "I have the result, only I do not yet know how to get to it." Einstein said that "the really valuable factor is intuition." In a lecture at Cornell, Millikan said that "Einstein's $h\nu$ formula was originally a mathematical hunch." Sommerfeld states that "the Bohr theory is not yet fully worked out mathematically; but is conceived intuitively." Gibbs was, of course, entirely deductive. Fermat's theorem was not proved.

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[1] "Atoms," VII (1923).

I have been told several times that Faraday's discovery of the laws of electrolysis was a triumph for the Baconian method. This is based on an absolute misconception of what Faraday actually did. From experiments with a friction machine charging a battery of Leyden jars which were then discharged through a galvanometer, Faraday concluded that the deflecting force of an electric current is probably directly proportional to the absolute quantity of electricity passed, at whatever intensity that electricity may be. Faraday then proved that the deflection produced when the jars were charged with thirty revolutions of the friction machine was the same as that produced by specified wires of zinc and platinum a specified distance apart if immersed five-eighths of an inch in a specified sulphuric acid for eight beats of his watch—about 3.2 seconds. This work was reported[1] in January, 1833.

"373. The following arrangements and results are selected from many that were made and obtained relative to chemical action. A platina wire one-twelfth of an inch in diameter, weighing two hundred and sixty grains, had the extremity rendered plain so as to offer a definite surface equal to a circle of the same diameter as the wire; it was then connected in turn with the conductor of the machine, or with the voltaic apparatus (369), so as always to form the positive pole, and at the same time retain a perpendicular position, that it might rest with its whole weight, upon the test paper to be employed. The test paper itself was supported upon a platina spatula, connected either with a discharging train (292), or with the negative wire of the voltaic apparatus, and it consisted of four thicknesses, moistened at all times to an equal degree in a standard solution of hydriodate of potassa (316).

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[1] Faraday: "Experimental Researches in Electricity," *1,* 103 (1839).

"374. When the platina wire was connected with the prime conductor of the machine, and the spatula with the discharging train, ten turns of the machine had such decomposing power as to produce a pale round spot of iodine of the diameter of the wire; twenty turns made a much darker mark, and thirty turns made a dark brown spot penetrating to the second thickness of the paper. The difference in effect produced by two or three turns, more or less, could be distinguished with facility.

"375. The wire and the spatula were then connected with the voltaic apparatus (369) the galvanometer also being included in the arrangement, and, a stronger acid having been prepared, consisting of nitric acid and water, the voltaic apparatus was immersed so far as to give a permanent deflection of the needle to the 5 1/3 division (372), the fourfold moistened paper[1] intervening as before. Then by shifting the end of the wire from place to place upon the test paper, the effect of the current for five, six, seven, or any number of beats of the watch (369) was observed and compared with that of the machine. After alternating and repeating the experiments of comparison many times, it was constantly found that this standard current of voltaic electricity, continued for eight beats of the watch, was equal in chemical effect to thirty turns of the machine; twenty-eight revolutions of the machine were sensibly too few.

"376. Hence it results that both in *magnetic deflection*

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[1] Of course the heightened power of the voltaic battery was necessary to compensate for the bad conductor now interposed.

(371) and in *chemical force,* the current of electricity of the standard voltaic battery for eight beats of the watch was equal to that of the machine evolved by thirty revolutions.

"377. It also follows that for this case of electro-chemical decomposition, and it is probable for all cases that the *chemical power, like the magnetic force* (366) *is in direct proportion to the absolute quantity of electricity* which passes."

Faraday's conclusion was right; but it does not follow necessarily from the experiments as described. He had proved, for this one case and with an experimental error of perhaps five per cent, that there was a definite relation between the quantity of electricity and the amount of chemical decomposition; but that would be equally true if the amount of chemical decomposition were proportional to some power of the quantity of electricity. The relation postulated by Faraday is the simplest one. It is possible that he overlooked the other relations; but I believe that he had guessed the result and that he was only concerned with proving the identity in chemical action of the electricity from the two sources.

Tyndall[1] lays stress continually on the intuitive and imaginative nature of Faraday's thinking. "Faraday has been called a purely inductive philosopher. A great deal of nonsense is, I fear, uttered in this land of England about induction and deduction. Some profess to befriend the one, some the other, while the real vocation of an investigator, like Faraday, consists in the incessant marriage of both. He was at this time full of the theory of Ampère, and it cannot be doubted that numbers of his experiments were executed merely to test his deductions from that theory."

"Faraday saw mentally the rotating disk, under the operation of the magnet, flooded with his induced currents, and from the known laws of interaction between current and magnets he hoped to deduce the motion observed by Arago. That hope he realised, showing by actual experiment that, when his disk rotated, currents passed through it, their position and direction being such as must, in accordance with the established laws of electro-magnetic action, produce the observed rotation."

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[1] "Faraday as a Discoverer," 23, 26, 28, 44, 73, 106, 121 (1868).

"At the suggestion of a mind fruitful in suggestions of a profound and philosophical character—I mean that of Sir John Herschel—Mr. Barlow, of Woolwich, had experimented with a rotating iron shell. Mr. Christie had also performed an elaborate series of experiments on a rotating iron disk. Both of them had found that when in rotation the body exercised a peculiar action upon the magnetic needle, which was not observed during quiescence; but neither of them was aware at the time of the agent which produced this extraordinary deflection. They ascribed it to some change in the magnetism of the iron shell and disk.

"But Faraday at once saw that his induced current must come into play here, and he immediately obtained them from an iron disk. With a hollow brass ball, moreover, he produced the effects obtained by Mr. Barlow. Iron was in no way necessary: the only condition of success being that the rotating body should be of a character to admit of the formation of currents in its substance: it must, in other words, be a conductor of electricity. The higher the conducting power the more copious were the currents. He now passes from his little brass globe to the globe of the earth. He plays like a magician with the earth's magnetism. He sees the invisible lines along which its magnetic action is exerted, and sweeping his wand across these lines evokes this new power.

Placing a simple loop of wire round a magnetic needle he bends its upper portion to the west: the north pole of the needle immediately swerves to the east; he bends his loop to the east, and the north pole moves to the west. Suspending a common bar magnet in a vertical position, he causes it to spin round its own axis. Its pole being connected with one end of a galvanometer wire, and its equator with the other end, electricity rushes round the galvanometer from the rotating magnet. He remarks upon the '*singular independence*' of the magnetism and the body of the magnet which carries it. The steel behaves as if it were isolated from its own magnetism. And then his thoughts suddenly widen, and he asks himself whether the rotating earth does not generate induced currents as it turns round on its own axis from west to east."

"His mind rises from the minute to the vast, expanding involuntarily from the smallest laboratory fact till it embraces the largest and grandest natural phenomena. In reality, however, he is at this time only clearing his way, and he continues laboriously to clear it for some time afterwards. He is digging the shaft, guided by that instinct towards the mineral lode which was to him a rod of divination."

"In the researches now under review the ratio of speculation and reasoning to experiment is far higher than in any of Faraday's previous works. Amid much that is entangled and dark we have flashes of wondrous insight and utterances which seem less the product of reasoning than of revelation."

"With that admirable instinct which always guided him, Faraday had seen that it was possible, if not probable, that the diamagnetic force acts with different degrees of intensity in different directions through the mass of a crystal. In his studies on electricity, he had sought an experimental reply to the question whether crystalline bodies had not different specific inductive capacities in different directions, but he failed to establish any difference of the kind. His first attempt to establish differences of diamagnetic action in different directions through bismuth, was also a failure; but he must have felt this to be a point of cardinal importance, for he returned to the subject in 1850, and proved that bismuth was repelled with different degrees of force in different directions."

"The manner in which Faraday himself habitually deals with his hypotheses is revealed in this lecture. He incessantly employed them to gain experimental ends, but he incessantly took them down, as an architect removes the scaffolding when the edifice is complete. 'I cannot but doubt,' says he, 'that he who as a mere philosopher has most power of penetrating the secrets of nature, *and guessing by hypothesis* at her mode of working, will also be most careful for his own safe progress and that of others, to distinguish the knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws.' Faraday himself, in fact, was always 'guessing by hypothesis,' and making theoretic divination the stepping-stone to his experimental results."

One knows of Joule as the man who determined the mechanical equivalent of heat and there is consequently an expectation of Joule turning out to be an accumulator. This is quite untrue. Joule did much more than determine the mechanical equivalent of heat. He had to convince people that there was such a thing to determine.[1] "To the present student endeavouring to enter fully into the history of the discovery, difficulties are presented by his own familiarity with expressions and terms, now part of his language, but which came into existence during the discovery, by his familiarity with those facts the discovery of which resulted in the generalization, and by his familiarity with facts almost innumerable, previously unknown, but which have been revealed as a consequence of the generalization.

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[1] Osborne Reynolds: "Memoir of James Prescott Joule," 17, 75, 109 (1892).

"The terms 'energy' and 'work' did not exist in the language of science in their present significance. The *vis viva* of a body, the product of the square of its velocity multiplied by its mass, had since the time of Newton been recognized as a mechanical quantity, and the term 'energy' had been applied to half this quantity by Young. On the other hand, 'work'—motion against resistance—expressed as the product of the distance, multiplied by the mean resistance overcome, although it was known to express the half of the change in the *vis viva* which takes place in a body moving against resistance, had never been recognized in the schools of mechanical philosophy as a fundamental measure of mechanical action, either as 'work' or by any other name."

"The fact that the early papers of Joule were, at the time, apparently ignored by the many eminent physicists then living, though apt to inspire the present reader with a feeling of astonishment, if not of indignation, at the generation for their prejudice and neglect, was, in truth, the highest tribute that could be paid to the greatness of the advance in philosophy which he had made."

"Joule's paper at the Oxford meeting made a great sensation. Faraday was there and was much struck with it, but did not enter fully into the new views. It was many years after that, before any of the scientific chiefs began to give their adhesion. It was not long after when Stokes told me [William Thomson] he was inclined to be a Joulite. Miller or Graham, or both, were for many years quite incredulous as to Joule's results, because they all depended on fractions of a degree of temperature—sometimes very small fractions. His boldness in making such large conclusions from such very small observational effects, is almost as noteworthy and admirable as his skill in extorting accuracy from them. I remember distinctly at the Royal Society, I think it was either Graham or Miller saying simply he did not believe Joule because he had nothing but hundredths of a degree to prove his case by."

Joule was the first to give the now accepted explanation of shooting stars—that they are meteorites rendered hot by the friction they meet on encountering the atmosphere. Joule was led to this explanation by his experiments on the heat developed by the friction of fluids. This was certainly a case of getting one's working hypothesis early in the game and this would not have appealed to Bacon at all.

Some people class Darwin as an accumulator because he spent so many years collecting data before he published anything; but it is the spirit in which the data are collected which counts and not the time spent in collecting them. A more serious difficulty is to be found in a statement by Darwin himself. "By collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject. My first note-book was opened in July, 1837. I worked on true Baconian principles, and without any theory, collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed inquiries, by conversation with skilful breeders and gardeners, and by extensive reading."

This would seem to be conclusive; but it contradicts other statements by Darwin and I believe that it is actually misleading. In the beginning there were no facts available and all that Darwin means to say is that he did not try to make a theory until he had collected a number of facts, which is perfectly proper. We get a more accurate conception of the way in which Darwin worked from Poulton's article in the Encyclopædia Britannica.[1]

"Soon after opening his note-book in July 1837, he began to collect facts bearing upon the formation of the breeds of domestic animals and plants and quickly saw 'that selection was the keystone of man's success. But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me.' Various ideas as to the causes of evolution occurred to him only to be successively abandoned. He had the idea of 'laws of change' which affected species and finally led to their extinction, to some extent analogous to the causes which bring about the development, maturity, and finally death of an individual. He also had the conception that species must give rise to other species or else die out, just as an individual dies unrepresented if it bears no offspring. These and other ideas, of which traces exist in his Diary, arose in his mind, together with perhaps some general conception of natural selection, during the fifteen months after the opening of his notebook.

In October, 1838, he read *Malthus on Population,* and his observations having long since convinced him of the struggle for existence, it at once struck him 'that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The result of this would be the formation of new species. Here then I had a theory by which to work.' In June 1842 he wrote out a sketch which two years later he expanded to an essay occupying 231 pages folio."

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[1] *7,* 841 (1910).

In other words Darwin collected facts for fifteen months, turning over in his mind possible explanations for them. Then he found "a theory by which to work," and he proceeded to test it for twenty years more, when he was forced into what he considered premature publication. That is most emphatically, getting one's working hypothesis early in the game and I, therefore, class Darwin as a guesser. This is in accord with Poulton's statement that the essential causes of Darwin's success were "the creative genius ever inspired by existing knowledge to build hypotheses by whose aid further knowledge could be won, the calm unbiased mind, the transparent honesty and love of truth which enabled him to abandon or to modify his own creations when they ceased to be supported by observation. The even balance between these powers was as important as their remarkable development."

Mendel is one of the doubtful cases. One can easily claim that he is a brilliant example of the Baconian method, having collected his data until the results were fairly obvious. One can also claim that he was sure in advance that there would be some statistical ratio for hybrids and that he searched for it, which would make him a guesser. We know so little about him that it is difficult to make a definite decision. Bateson[1] gives a fair summary of what little we know about Mendel's starting point.

"His success is due to the clearness with which he thought out the problem. Being familiar with the works of Gaertner and the other experimental breeders, he surmised that their failure to reach definite and consistent conclusions was due to a want of precise and continued analysis. In order to obtain a clear result he saw that it was absolutely necessary to start with pure-breeding, homogeneous materials, to consider each character separately, and on no account to confuse the different generations together. Lastly he realised that the progeny from distinct individuals must be separately recorded. All these ideas were entirely new in his day. When such precautions had been observed, he anticipated that a regular result would be attainable if the experiments were carried out on a sufficient scale."

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[1] "Mendel's Principles of Heredity," 7 (1909).

Pasteur was a brilliant guesser and a great fighter. As he himself said: "Without theory, practice is but routine born of habit. Theory alone can bring forth and develop the spirit of invention." When he started on the question of alcoholic fermentation, he observed that the juice from the sick vats contained special rods. After trying several hypotheses which did not prove right, he finally decided that "the little rods in the juice of the sick vats are alive, and it is *they* that make the acid of sour milk—the rods fight with the yeasts perhaps, and get the upper hand. They are the ferment of sour-milk-acid, just as the yeasts must be the ferment of the alcohol."

In regard to the work on lactic acid fermentation Duclaux[1] says that "this memoir is full of suggestion and, strangely enough, all these propositions which were so new and so bold for the epoch were announced *de plano* almost carelessly, with the tranquil confidence of a man sure of his facts, and to whom, if one did not know him, one might even have attributed malicious intentions, he showed so much apathy. It is only at the end of his memoir that he admits that nothing of all this has been demonstrated."

In another passage,[2] Duclaux quotes Pasteur's own words. "I take the liberty of recalling to my confrère, M. Blanchard, that the illusions of an experimenter form a great part of his power. These are the preconceived ideas which serve to guide him. Many of them vanish in the long path which he must travel, but one fine day he discovers and proves that some of these are adequate to the truth. Then he finds himself master of new facts and new principles, the application of which, sooner or later, bestows their benefits."

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[1] "Pasteur, the History of a Mind," 72 (1920).

[2] p. 280.

The case of anthrax is similar in principle to that of the acid fermentation, though very much more striking so far as the public was concerned.[1]

"The confusion of ideas on the origin of contagious and epidemic diseases was about to be suddenly enlightened; Pasteur had now taken up the study of the disease known as charbon or splenic fever. This disease was ruining agriculture; the French provinces of Beauce, Brie, Burgundy, Nivernais, Berry, Champagne, Dauphiné, and Auvergne paid a formidable yearly tribute to this mysterious scourge. In the Beauce, for instance, twenty sheep out of every hundred died in one flock; in some parts of Auvergne the proportion was ten or fifteen percent, some times even twenty-five, or fifty percent. At Provins, at Meaux, at Fontainebleau, some farms were called *charbon farms;* elsewhere, certain fields or hills were looked upon as accursed and an evil spell seemed to be thrown over flocks bold enough to enter those fields or ascend those hills. Animals stricken with this disease almost always died in a few hours; sheep were seen to lag behind the flock, with drooping head, shaking limbs and gasping breath; after a rigor and some sanguinolent evacuations, occurring also through the mouth and nostrils, death supervened, often before the shepherd had had time to notice the attack. The carcase rapidly became distended, and the least rent in the skin gave issue to a flow of black, thick and viscid blood, hence the name

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[1] R. Vallery-Radot: "The Life of Pasteur," 257, 259 (1923).

Anthrax given to the disease. It was also called splenic fever, because necropsy showed that the spleen had assumed enormous dimensions. If that were opened, it presented a black and liquid pulp. In some places the disease assumed a character of extreme virulence; in the one district of Novgorod, in Russia, 56,000 head of cattle died of splenic infection between 1867 and 1870. Horses, oxen, cows, sheep, everything succumbed, as did also 528 persons, attacked by the contagion under divers forms. A pin prick or a scratch is sufficient to inoculate shepherds, butchers, knackers, or farmers with the malignant pustule."

"Pasteur tackled the subject. A little drop of the blood of an animal which had died of anthrax—a microscopic drop—was laid, sown, after the usual precautions to ensure purity, in a sterilized balloon which contained neutral or slightly alkaline urine. The culture medium might equally be common household broth, or beer-yeast-water, either of them neutralized by potash. After a few hours, a sort of flake was floating in the liquid; the bacteridia could be seen, not under the shape of short broken rods, but with the appearance of filaments, tangled like a skein; the culture medium being highly favourable, they were growing longer. A drop of that liquid abstracted from the first vessel, was sown into a second vessel, of which one drop was again placed into a third, and so on, until the fortieth flask; the seed of each successive culture came from a tiny drop of the preceding one. If a drop from one of those flasks was introduced under the skin of a rabbit or a guinea-pig, splenic fever and death immediately ensued, with the same symptoms and characteristics as if the original drop of blood had been inoculated.

In the presence of the results from those successive cultures, what became of the hypothesis of an inanimate substance contained in the first drop of blood? It was now diluted in a proportion impossible to imagine. It would therefore be absurd, thought Pasteur, to imagine that the last virulence owed its power to a virulent agent existing in the original drop of blood; it was to the bacteridium, multiplied in each culture, and to the bacteridium alone, that this power was due; the life of the bacteridium had made the virulence. "Anthrax is therefore," Pasteur declared, "the disease of the bacteridium, as trichinosis is the disease of the trichina, as itch is the disease of its special acarus, with this circumstance, however, that in anthrax, the parasite can only be seen through a microscope, and very much enlarged." After the bacteridium had presented those long filaments, within a few hours, two days at the most, another spectacle followed; amidst those filaments, appeared the oval shapes, the germs, spores, or seeds, pointed out by Dr. Koch. Those spores, sown in broth, reproduced in their turn the little packets of tangled filaments, the bacteria. Pasteur reported that one single germ of bacteridium in the drop which is sown multiplies during the following hours and ends by filling the whole liquid with such a thickness of bacteridia that, to the naked eye, it seems that carded cotton has been mixed with the broth."

Lister's work was entirely deductive. His original idea was that oxygen caused suppuration of wounds, because the presence of air was apparently necessary. He was quick to perceive the importance of Pasteur's work on microorganisms in the air and he followed out this line of attack.[1]

"Lister did for the craft of surgery what John Hunter had done for its science. When he first began his work, operations were few owing to the danger of putrefaction in the wound, followed in almost all cases by death. Even the simplest operation was a great anxiety to the surgeon, from the ever-present fear of suppuration developing. Lister's discovery was very gradual. His earliest surgical inquiries dealt with inflammation and the coagulation of the blood; but his chief interest lay always in the problem of the healing of wounds. He had arrived at the conclusion that the essential cause of suppuration in wounds was decomposition brought about by the atmosphere acting upon blood and serum retained in them, or upon portions of destroyed tissues; but, since oxygen was considered to be the agent causing this putrefaction, it appeared hopeless to devise a method by which suppuration might be prevented. But when Pasteur had shown that putrefaction was caused by minute organisms suspended in the air, a method of prevention at once came to his mind, to apply to the wound some substance which would destroy the micro-organisms without injuring the body tissues. Still later he developed a method by which the organisms might be destroyed before they had even entered the wound. Around every step of his advance, fierce controversy raged; the scepticism of early contemporaries was stupid, unimaginative, and petty. But the history of science frequently discloses this bitter opposition to new truths, as in the case of Harvey, Pasteur, and other famous men. Lister's answer was unfaltering continuance in inquiry and experiment, with demonstration of his results. The heavily infected wounds seen during the War have enabled us to realise much more acutely the problems which confronted Lister at the beginning of his work, and have increased our admiration for the way he overcame them. Although Lister sought to destroy the organisms which might enter a wound, yet he was not blind to the natural resistance of the body's cells to infection, so that a natural step was the development of aseptic surgery in which organisms are prevented from entering a wound so far as possible and any that do can then be dealt with by the body's own bactericidal forces. There is no real clash between 'antiseptic' and 'aseptic' methods, for no surgeon ever practised with success a method which omitted the use of agents for the destruction of organisms. The consequences of Lister's work were many and far-reaching: when the few operations, which were practised in those days, became safe, it was obvious that others might be attempted, and thus has grown up the science and art of modern surgery. Ovariotomy was one of the first operations to be made safe; and once it was found that the abdomen could be safely opened, a vast field of usefulness was before the surgeon. The cranial and thoracic cavities then became accessible to surgical methods of treatment, so that nowadays almost all parts of the body can be safely submitted to surgical operation. Not the least of the debts we owe to Lister is the curability of cancer if complete surgical removal is practised in the early stages of the disease. We may almost claim that the full effect of Lister's work is now accomplished. The art of surgery is far in advance of the sciences on which its future progress depends. The great search must be for methods of applying new discoveries in other sciences to the study of disease.

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[1] Sir Berkeley Moynihan: Nature, *119*, 572 (1927).

No one would question the propriety of putting Helmholtz, van't Hoff, Arrhenius, J. J. Thomson, and Rutherford among the guessers. It is interesting to recall that Helmholtz's paper on the "Conservation of Energy"[1] was called unintelligible speculations by Poggendorff.

Dewar is a striking example of an accumulator who was very versatile. When he studied low-temperature problems, for instance, he covered the ground very thoroughly. It was not merely a question of developing improved methods of liquefying gases and of determining melting-points, boiling-points, and densities. Dewar studied specific heats, latent heats of vaporization, diffusion, adsorption by charcoal, optical and magnetic properties, color, photochemical reactions, the effects of low temperatures on bacteria and on electrical resistance, etc., etc. It is a real pleasure to note how many sides Dewar saw to a problem. While one cannot fail to be impressed by the brilliancy and versatility of Dewar's work and by the remarkable manipulative skill that he showed, there is surprisingly little in the way of theory. Dewar was an accumulator and not a guesser. He was intensely interested in experimentation; but he did not care at all for the theoretical bearing of his experiments. He did a great deal of work on the adsorption of gases; but he was not interested in the laws of adsorption. This characteristic appears very strongly in his work on soap films. The results are fascinating and are veritable triumphs of experimental ingenuity. Bubbles were blown four feet in diameter which lasted several hours; one bubble, 46 cm. in diameter, lasted sixty-three days; and a horizontal black film, 20 cm. in diameter, lasted for a year. Dewar gives all details; but he draws no theoretical conclusions from them and makes no effort to do so.

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[1] (1847).

No classification of this sort gives a sharp dividing line. One can find all sorts of gradations between the two extremes and each one of us can pick out men whom he finds difficult to put definitely in one or the other of the two groups. For me Ramsay is on the line. Some days I decide to classify him as a guesser and on other days he seems certainly an accumulator. Perhaps it will save trouble to call him a transition personage.

While it is true that the overwhelming majority of the important theoretical advances have been made by guessers and not by accumulators, it would be ridiculous to claim that no important discoveries stand to the credit of the accumulators. Bumstead[1] says that "accurate measurements, however, do sometimes produce brilliant discoveries—when they fall into the right hands. A classical example of this is the discovery of argon by Lord Rayleigh, as the result of a quite prosaic undertaking to redetermine with great accuracy the density of nitrogen." The hydrogenation of oils and much of the catalytic work of today depends on the experiments of Sabatier, who is a typical accumulator. Millikan's work on cosmic rays, which has interested the public so much, must be credited to the accumulators. There are doubtless other important discoveries, with which I am not familiar and which belong in the same class; but the total number in this group is relatively small. On the other hand the scientific public has been so encouraged to believe in the tremendous value of exact measurements that a really first-class accumulator is practically certain of being awarded a Nobel prize. For instance, Millikan says that "progress in physics follows invention of new measuring apparatus and the proof from exact measurements that formula is not adequate. Instances are isotopes and the fact that 4H is not exactly equal to He in its bearing on the source of the sun's radiation."

There is nothing especially new in this criticism of the Baconian method. People have been doing it from the time that Bacon put forward his views; but it has had surprisingly little effect. Harvey said that "the Lord Chancellor writes of science—like a Lord Chancellor."

Whewell[2] is strong against the Baconian method and an enthusiastic supporter of the doctrine of guessing. "When mere position, and number, and resemblance, will no longer answer the purpose of enabling us to connect the facts, we call in other Ideas, in such cases more efficacious, though less obvious.

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[1] Woodruff: "The Development of the Sciences," 56 (1923).

[2] "Novum Organum Renovatum," 63, 78 (1858).

"But how are we, in these cases, to discover such Ideas, and to judge which will be efficacious, in leading to a scientific combination of our experimental data? To this question, we must in the first place answer, that the first and great instrument by which facts so observed with a view to the formation of exact knowledge, are combined into important and permanent truths, is that peculiar Sagacity which belongs to the genius of a Discoverer; and which, while it supplies those distinct and appropriate Conceptions which lead to its success, cannot be limited by rules, or expressed in definitions. It would be difficult or impossible to describe in words the habits of thought which led Archimedes to refer the conditions of equilibrium on the Lever to the Conception of pressure, while Aristotle could not see in them anything more than the results of the strangeness of the properties of the circle:—or which impelled Pascal to explain by means of the Conception of the *weight of air,* the facts which his predecessors had connected by the notion of nature's horrour of a vacuum; or which caused Vitello and Roger Bacon to refer the magnifying power of a convex lens to the bending of the rays of light towards the perpendicular by refraction, while others conceived the effect to result from the matter of medium, with no consideration of its form. These are what are commonly spoken of as felicitous and inexplicable strokes of inventive talent; and such, no doubt, they are. No rules can ensure to us similar success in new cases; or can enable men who do not possess similar endowments, to make like advances in knowledge.

"Yet still, we may do something in tracing the process by which such discoveries are made; and this it is here our business to do. We may observe that these, and the like discoveries, are not improperly described as happy *Guesses;* and that Guesses, in these as in other instances, imply various suppositions made, of which some one turns out to be the right one. We may, in such cases, conceive the discoverer as inventing and trying many conjectures, till he finds one which answers the purpose of combining the scattered facts into a single rule. The discovery of general truths from special facts is performed, commonly at least, and more commonly than at first appears, by the use of a series of suppositions, or *Hypotheses,* which are looked at in quick succession, and of which the one which really leads to truth is rapidly detected, and when caught sight of, firmly held, verified, and followed to its consequences. In the minds of most discoverers, this process of invention, trial, and acceptance or rejection of the hypothesis, goes on so rapidly that we cannot trace it in its successive steps. But in some instances, we can do so; and we can also see that the other examples of discovery do not differ essentially from these. The same intellectual operations take place in other cases, although this often happens so instantaneously that we lose the trace of the progression. In the discoveries made by Kepler we have a curious and memorable exhibition of this process in its details. Thanks to his communicative disposition, we know that he made nineteen hypotheses with regard to the motion of Mars, and calculated the results of each, before he established the true doctrine, that the planet's path is an ellipse. We know, in like manner, that Galileo made wrong suppositions respecting the laws of falling bodies, and Mariotte, concerning the motion of water in a siphon, before they hit upon the correct view of these cases.

"But it has very often happened in the history of science that the erroneous hypotheses which preceded the discovery of the truth have been made, not by the discoverer himself, but by his precursors; to whom he thus owed the service, often an important one in such cases, of exhausting the most tempting forms of errour. Thus the various fruitful suppositions by which Kepler endeavoured to discover the law of refraction, led the way to its real detection by Snell; Kepler's numerous imaginations concerning the forces by which the celestial motions are produced—his 'physical reasonings' as he termed them,—were a natural prelude to the truer physical reasonings of Newton. The various hypotheses by which the suspension of vapour in air had been explained, and their failure, left the field open for Dalton with his doctrine of the mechanical mixture of gases. In most cases, if we could truly analyze the operation of the thoughts of those who make, or who endeavour to make discoveries in science, we should find that many more suppositions pass through their minds than those which are expressed in words; many a possible combination of conceptions is formed and soon rejected. There is a constant invention and activity, a perpetual creating and selecting power at work, of which the last results only are exhibited to us. Trains of hypotheses are called up and pass rapidly in review; and the judgment makes its choice from the varied group.

"It would, however, be a great mistake to suppose that the hypotheses, among which our choice thus lies, are constructed by an enumeration of obvious cases, or by a wanton alteration of relations which occur in some first hypothesis.

It may, indeed, sometimes happen that the proposition which is finally established is such as may be formed, by some slight alteration, from those which are justly rejected. Thus Kepler's elliptical theory of Mars's motions involved relations of lines and angles much of the same nature as his previous false suppositions: and the true law of refraction so much resembles those erroneous ones which Kepler tried, that we cannot help wondering how he chanced to miss it. But it more frequently happens that new truths are brought into view by the application of new Ideas, not by new modifications of old ones. The cause of the properties of the Lever was learnt, not by introducing any new *geometrical* combination of lines and circles, but by referring the properties to genuine *mechanical* Conceptions. When the Motions of the Planets were to be explained, this was done, not by merely improving the previous notions, of cycles of time, but by introducing the new conception of *epicycles* in space. The doctrine of the Four Simple Elements was expelled, not by forming any new scheme of elements which should impart according to new rules, their sensible qualities to their compounds, but by considering the elements of bodies as *neutralizing* each other. The Fringes of Shadows could not be explained by ascribing new properties to the single rays of light, but were reduced to law by referring them to the *interference* of several rays.

"Since the true supposition is thus very frequently something altogether diverse from all the obvious conjectures and combinations, we see here how far we are from being able to reduce discovery to rule, or to give any precepts by which the want of real invention and sagacity shall be supplied. We may warn and encourage these faculties when they exist, but we cannot create them, or make great discoveries when they are absent. . . .

"To discover a Conception of the mind which will justly represent a train of observed facts is, in some measure, a process of conjecture, as I have stated already; and, as I then observed, the business of conjecture is commonly conducted by calling up before our minds several suppositions, and selecting that one which most agrees with what we know of the observed facts. Hence he who has to discover the laws of nature may have to invent many suppositions before he hits upon the right one; and among the endowments which lead to his success, we must reckon that fertility of invention which ministers to him such imaginary schemes, till at last he finds the one which conforms to the true order of nature. A facility in devising hypotheses, therefore, is so far from being a fault in the intellectual character of a discoverer, that it is, in truth, a faculty indispensable to his task. It is, for his purposes, much better that he should be too ready in contriving, too eager in pursuing systems which promise to introduce law and order among a mass of unarranged facts, than that he should be barren of such inventions and hopeless of such success. Accordingly, as we have already noticed, great discoverers have often invented hypotheses which would not answer to all the facts, as well as those which would; and have fancied themselves to have discovered laws, which a more careful examination of the facts overturned.

"The tendencies of our speculative nature, carrying us onwards in pursuit of symmetry and rule, and thus producing all true theories, perpetually show their vigour by overshooting the mark. They obtain something by aiming at much more. They detect the order and connexion which exist, by conceiving imaginary relations of order and connexion which have no existence. Real discoveries are thus mixed with baseless assumptions; profound sagacity is combined with fanciful conjecture; not rarely, or in peculiar cases, but commonly, and in most cases; probably in all, if we could read the thoughts of discoverers as we read the books of Kepler. To try wrong guesses is, with most persons, the only way to hit upon right ones. The character of the true philosopher is, not that he never conjectures hazardously, but that his conjectures are clearly conceived, and brought into rigid contact with facts. He sees and compares distinctly the Ideas and Things;—the relations of his notions to each other and to phenomena. Under these conditions, it is not only excusable, but necessary for him, to snatch at every semblance of general rule,—to try all promising forms of simplicity and symmetry.

"Hence advances in knowledge are not commonly made without the previous exercise of some boldness and licence in guessing. The discovery of new truths requires, undoubtedly, minds careful and scrupulous in examining what is suggested; but it requires, no less, such as are quick and fertile in suggesting. What is Invention, except the talent of rapidly calling before us the many possibilities, and selecting the appropriate one? It is true that, when we have rejected all the inadmissible suppositions, they are often quickly forgotten; and few think it necessary to dwell on these discarded hypotheses and on the processes by which they were condemned. But all who discover truths, must have reasoned upon many errors to obtain each truth; every accepted doctrine must have been one chosen out of many candidates. If many of the guesses of philosophers of bygone times now appear fanciful and absurd, because time and observation have refuted them; others, which were at the time equally gratuitous, have been confirmed in a manner which makes them appear marvellously sagacious. To form hypotheses and then to employ much labour and skill in refuting them, if they do not succeed in establishing them, is a part of the usual process of inventive minds. Such a proceeding belongs to the *rule* of the genius of discovery, rather than (as has often been thought in modern times) to the exception."

In a later book Whewell[1] says: "Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin:—some fortunate cast of intellect rising above all rules. No maxims can be given which will inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius."

Brodie[2] wrote: "Lastly, physical investigations, more than anything besides, help to teach us the actual value and right use of the Imagination, of that wondrous faculty, which, left to ramble uncontrolled, leads us astray into a wilderness of perplexities and errors, a land of mists and shadows; but which, properly controlled by experience and reflection, becomes the noblest attribute of man; the source of poetic genius; the instrument of discovery in Science, without the aid of which Newton would never have invented fluxions, nor Davy have decomposed the earths and alkalies, nor would Columbus have found another Continent."

Tyndall[3] takes practically the same view. "Mathematics and physics have long been accustomed to coalesce. For, no matter how subtle a natural phenomenon may be, whether we observe it in the region of sense, or follow it into that of imagination, it is in the long run reducible to mechanical laws. But the mechanical data once guessed or given, mathematics becomes all-powerful as an instrument of deduction.

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[1] "Philosophy of Discovery," 44 (1860).

[2] Proc. Roy. Soc., *10,* 165 (1860).

[3] "Fragments of Science," 410, 426, 546 (1884).

The command of Geometry over the relations of space, and the far-reaching power which Analysis confers, are potent both as means of physical discovery, and of reaping the entire fruits of discovery. Indeed, without mathematics, expressed or implied, our knowledge of physical science would be both friable and incomplete.

"Side by side with the mathematical method we have the method of experiment. Here from a starting-point furnished by his own researches or those of others, the investigator proceeds by combining intuition and verification. He ponders the knowledge he possesses, and tries to push it further; he guesses and checks his guesses; he conjectures, and confirms or explodes his conjectures. These guesses and conjectures are by no means leaps in the dark; for knowledge once gained casts a faint light beyond its own immediate boundaries. There is no discovery so limited as not to illuminate something beyond itself. The force of intellectual penetration into this penumbral region which surrounds actual knowledge is not, as some seem to think, dependent upon method, but upon the genius of the investigator. The profoundest minds know best that Nature's ways are not at all times their ways, and that the brightest flashes in the world of thought are incomplete until they have been proved to have their counterpart in the world of fact. Thus the vocation of the true experimentalist may be defined as the continued exercise of spiritual insight and its incessant correction and realization. His experiments constitute a body of which his purified insight is, as it were, the soul. . . .

"There are Tories even in science who regard Imagination as a faculty to be feared and avoided rather than employed. They have observed its action in weak vessels and are unduly impressed by its disasters. But they might with equal justice point to exploded boilers as an argument against the use of steam. Nourished by knowledge patiently won; bounded and conditioned by co-operant Reason; imagination becomes the prime mover of the physical discoverer. Newton's passage from a falling apple to a falling moon was, at the outset, a leap of prepared imagination. In Faraday the exercise of this faculty preceded all his experiments, and its function has been impressively set forth by Brodie. When William Thomson [Lord Kelvin] tries to place the ultimate particles of matter between his compass points and to apply to them a scale of millimetres, it is an act of the scientific imagination. And in much that has recently been said about protoplasm and life we have the outgoings of this faculty guided and controlled by the known analogies of science. In fact, without this power, our knowledge of nature would be a mere tabulation of co-existences and sequences. . . .

"First of all, I am blamed for crossing the boundary of experimental evidence. This, I reply, is the habitual action of the scientific mind—at least of that portion of it which applies itself to physical investigation. Our theories of light, heat, magnetism, and electricity, all imply the crossing of this boundary. My paper on the 'Scientific Use of the Imagination' and my 'Lectures on Light,' illustrate this point in the amplest manner; and, in the brief discourse which follows this Address, I have sought incidentally to make clear that in physics the experiential incessantly leads to the extra-experiential; that out of experience there always grows something finer than mere experience; and that in their different powers of extension consists, for the most part, the difference between the great and the mediocre investigator. The kingdom of science, then, cometh not by observation and experiment alone, but is completed by fixing the roots of observation and experiment in a region inaccessible to both, and in dealing with which we are forced to fall back upon the picturing power of the mind."

Woodruff[1] quotes Huxley as saying that "it is a favorite popular delusion that the scientific enquirer is under a sort of moral obligation to abstain from going beyond that generalization of observed facts which is absurdly called 'Baconian' induction. But any one who is practically acquainted with scientific work is aware that those who refuse to go beyond fact, rarely get as far; and any one who has studied the history of science knows that almost every step therein has been made by the 'Anticipation of Nature,' that is, by the invention of hypotheses, which, though verifiable, often had very little foundation to start with; and not infrequently, in spite of a long career of usefulness, turned out to be wholly erroneous in the long run."

G. K. Gilbert[2] says: "It is the province of research to discover the antecedents of phenomena. This is done by the aid of hypothesis. A phenomenon having been observed, or a group of phenomena having been established by empiric classification, the investigator invents an hypothesis in explanation. He then devises and applies a test of the validity of the hypothesis. If it does not stand the test, he discards it and invents a new one. If it survives the test, he proceeds at once to devise a second test. And he thus continues until he finds an hypothesis that remains unscathed after all the tests his imagination can suggest.

"This, however, is not his universal course, for he is not restricted to the employment of one hypothesis at a time. There is indeed an advantage in entertaining several at once, for then it is possible to discover their mutual antagonisms and inconsistencies, and to devise crucial tests,—tests which will necessarily debar some of the hypotheses from further consideration. The process of testing is then a process of elimination, at least until all but one of the hypotheses have been disproved.

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[1] "The Development of the Sciences," 217 (1925).

[2] Am. J. Sci., (3) *31*, 286, 287 (1886).

"In the testing of hypotheses lies the prime difference between the investigator and the theorist. The one seeks diligently for the facts which may overthrow his tentative theory, the other closes his eyes to these and searches only for those which will sustain it.

"Evidently, if the investigator is to succeed in the discovery of veritable explanations of phenomena, he must be fertile in the invention of hypotheses and ingenious in the application of tests. The practical questions for the teacher are, whether it is possible by training to improve the guessing faculty, and if so, how it is to be done. To answer these, we must give attention to the nature of the scientific guess considered as a mental process. Like other mental processes, the framing of hypotheses is usually unconscious, but by attention it can be brought into consciousness and analyzed."

"The great investigator is primarily and preëminently the man who is rich in hypotheses. In the plenitude of his wealth he can spare the weaklings without regret; and having many from whom to select, his mind maintains a judicial attitude. The man who can produce but one, cherishes and champions that one as his own, and is blind to its faults. With such men, the testing of alternative hypotheses is accomplished only through controversy. Crucial observations are warped by prejudice, and the triumph of the truth is delayed."

Gregory[1] says that "Bacon drew up the rules by which he considered Nature should be studied, but he treated almost with contempt all progress accomplished without the use of his prescription, and he persistently rejected the Copernican theory, though it formed the best possible example of the application of his own system of collecting observations and arriving at conclusions from them. Few natural philosophers who came after him took heed of his artificial process of discovery; and there is little evidence that the method assisted in the advance of science in any way. Newton never mentioned Bacon or his system, though he was born and educated after its publication; and a study of the progress of science fails to furnish sufficient reason for believing that Bacon's 'Novum Organum' has been either a powerful source of inspiration or has provided the formula by which natural knowledge has been increased. It is, indeed, a mistake to suppose that all scientific investigation must proceed from the general to the particular according to a prescribed formula, or be determined by any like hard-and-fast principle. Devotion to such doctrines has often led men astray and is always an undesirable obsession."

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[1] "Discovery," 134 (1923).

Mellor[1] discusses what he calls Newton's inductive [?] method. "Here the attempt is made to infer the hidden generalization from the consequences of the assumption (hypothesis) what that generalization is. The process is sometimes called *a posteriori* reasoning. This method of investigation was extensively employed with glorious results by Isaac Newton, although it had been advocated by Aristotle two thousand years earlier. Francis Bacon, indeed, before Newton's time, protested against anticipating nature by hypotheses; but the greatest triumphs of modern science have been won by the application of the Newtonian method, while the Baconian method has been singularly unfruitful.

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[1] "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1,* 18 (1922).

Francis Bacon's failure in the practice of his own method was complete."

Mellor quotes De Morgan as saying: "According to Francis Bacon, facts are made to make theories *from*, and according to Isaac Newton, to try ready-made theories by." Woodruff[1] also says a good word for the Aristotelian method. "The climax of the scientific Renaissance involved a turning away from the authority of Aristotle and an adoption of the Aristotelian method of observation and induction."

Two generations ago, Claude Bernard,[2] quoting de Maistre, agreed "that they who make the most discoveries in science know Bacon least, while those who read and ponder him, like Bacon himself, have poor success."

Wright[3] emphasizes very properly the stress that Bacon laid on experiments; but he is, equally properly, not enthusiastic over the Baconian method itself. "Bacon may indeed have had an idea of the way the human mind works, but his chief preoccupation seems to have been with the way he thought it ought to work. Yet his own inductive method did much, a very great deal indeed subsequently to guide the natural mind of man into more profitable ways of working, to broaden the basis on which it must work; but without deductions, theories, hypotheses, it will not work at all and this Aristotle well understood. Apparently neither Bacon nor Newton did. [This is an error as regards Newton.] They may have thought so, but there was nothing wrong with Aristotle except the Aristotelians who preceded Bacon. Bacon as well as the Baconians were largely in error, not only as to Aristotle, but as to the natural workings of the human mind."

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[1] "The Development of the Sciences," 221 (1923).

[2] "An Introduction to the Study of Experimental Medicine," translated by Henry Copley Greene.

[3] Scientific Monthly, *26,* 38 (1928).

This mass of evidence might lead one to infer that it is flogging a dead horse to belabor the Baconian method. Unfortunately, this is not the case. So far as I can judge, the belief in the Baconian method is just as strong today and just as wide-spread as it has ever been.

Although Whewell has expressed his disbelief in the Baconian method, he begins his "Novum Organum Renovatum" with the following quotation from Herschel:—"It is to our immortal countryman, Bacon, that we owe the broad announcement of this grand and fertile principle; and the development of the idea, that the whole of natural philosophy consists entirely of a series of inductive generalizations, commencing with the most circumstantially stated particulars, and carried up to universal laws, or axioms, which comprehend in their statements every subordinate degree of generality; and of a corresponding series of inverted reasoning from generals to particulars, by which these axioms are traced back into their remotest consequence, and all particular problems deduced from them; as well as those by whose immediate considerations we rose to their discovery, as those of which we had no previous knowledge."

Only a few years ago Sir William Pope[1] said, in an address, that at the end of the sixteenth century Francis Bacon "accentuated, though he did not himself discover, the principles of the modern method of scientific enquiry. Although Bacon himself was a philosopher and not an experimenter, and although he made the most ineffective use of the tools which he did so much to perfect, his authority as Lord Chancellor of England and his eloquence as a writer gave a tremendous impetus to the experimental method of scientific inquiry, and may be said, for all practical purposes, to mark him as the founder of modern science."

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[1] Chemistry and Industry, *42,* 53 (1923).

Even worse is the statement by Westaway.[1] "There is one fundamental difference between English and German scholars. Taught by Bacon, English scholars seem to have acquired an instinctive desire to accumulate all possible facts before attempting to frame anything of the nature of a general law. But German thinkers tend to generalize before the accumulated facts afford the necessary justification. Their curious love of abstraction and their desire to deduce a whole universe from a few general propositions, constantly lead to their illegitimate use of deductive reasoning; they seem to be unsuspicious of the dangers of loosely establishing generalities."

This is about as wrong as a paragraph can be. Such great Englishmen as Harvey, Newton, Young, Dalton, Davy, Faraday, Joule, Darwin, Lister, J. J. Thomson, and Rutherford were guessers, and generalized long before the experimental data really justified it. On the other hand the German physicists and chemists have not been, as a rule, great generalizers. Their most striking characteristics have been the ability to recognize the importance of a generalization when it had been made by somebody else, and to develop the consequences and applications of the generalization in all its details.

As late as the end of April Peltier[2] said that "while many of Aristotle's observations are sound, his deductive reasoning on natural phenomena probably more than any other factor held in abeyance the adoption of the experimental or inductive methods in science." The mistake here is the common one of making experimental synonymous with inductive.

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[1] "Science and Theology," 26, 151 (1920).

[2] Science, (2) *68,* 192 (1928).

The Character Education Institute of Chevy Chase, Maryland, has sent out an enormous number of yellow slips dealing with what it calls the scientific methods. This Institute lists twenty-five "intellectual immoralities," number four being "generalizing beyond one's data." It was also stated that letters of advice are asked from all interested. Being still ignorant enough to believe that people sometimes mean what they say, I wrote to the unknown Institute saying that I thought that "Intellectual Immorality No. 4" should read "Not generalizing beyond one's data." I got back a letter saying: "Don't you think that hard and fast rules are a mistake," or words to that effect. I quite agree; but it would have been well if Mr. Milton Fairchild had thought of that before publishing his twenty-five "intellectual immoralities."

I have shown why I believe it to be better for a scientific man to try to be a guesser rather than an accumulator—if he can. No method of research is fool-proof and the obvious danger about the Aristotelian method is that one may become infatuated with a given working hypothesis and warp the facts to fit the hypothesis. This is a real danger but not so serious a one as many people think. The best protection against this source of error is to have several working hypotheses and to shift from one to the other with great readiness. Michael Faraday said once: "The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examinations; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized."

If a man like Faraday was pleased if he guessed right once out of ten times, most of us can rejoice if we make one successful guess in a hundred. If one really believes that, there is not much danger of clinging unfairly to any working hypothesis.

A more real danger, because most people do not know that it exists, is the belief that a working hypothesis must be right if it describes the facts accurately. This is not sufficient. One is safe only if one knows that no other hypothesis will disturb the facts equally well. There have been a number of cases in the last fifty years in which two entirely different hypotheses led to the same result. Three typical examples may be cited. Goodwin[1] made some admirable calculations on the electromotive forces of concentration cells, starting with the explicit assumption that the formula of mercurous chloride is HgCl. While I have mislaid the reference, I am quite certain that Ostwald stated somewhere that Goodwin had proved the formula of mercurous chloride to be HgCl as completely as anything can be proved in this world. Later, it developed that the formula for mercurous nitrate is $Hg_2(NO_3)_2$ and not $HgNO_3$, from which it follows that the formula for mercurous chloride should be written Hg_2Cl_2.

This was very disconcerting until it was found that Goodwin's equations came out the same way no matter what formula was assumed for mercurous chloride. Of course, somebody should have seen that in the beginning; but we are dealing with things as they are, and nobody did.

Some years ago Michelson[2] studied the colors of iridescent insects and of iridescent feathers, such as the tail feathers of the peacock, the throat feathers of the humming-bird, etc. He made such a difficult series of tests that nobody has yet had the courage to repeat them. These tests agreed with the working hypothesis that the colors were due to solid pigments showing selective reflection, analogous to magenta, for instance, and Michelson consequently concluded that the iridescent colors were due to solid pigments giving selective reflection. It has proved impossible, however, to extract any bright-colored pigment from any of the iridescent portions of insects and feathers. It has also been shown conclusively that these colors are due to thin films and not to solid pigments. Consequently, it follows that Michelson's tests did not enable him to differentiate between solid pigments showing selective reflection and colors due to thin films. This has not yet been shown, apparently because some of the physicists are afraid to question the accuracy of Michelson's work and because the others are not especially interested one way or the other.

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[1] Z. physik. Chem., *13*, 577 (1894).

[2] Phil. Mag. (6) *21*, 554 (1911).

When hydrochloric acid or caustic soda is added to gelatine and water, there is a disappearance of hydrogen ions in the first case and of hydroxyl ions in the second case. There is no dispute about the facts, but only about the interpretation of them. This disappearance of hydrogen or hydroxyl ions might be due to the formation of a salt, with amphoteric gelatine, gelatine hydrochloride or sodium gelatinate as the case might be. It might also be due to strong adsorption of hydrogen or hydroxyl ions by gelatine with no formation of stoichiometric compounds. The two conceptions are diametrically opposed. The first postulates the formation of definite chemical compounds with stoichiometric relations; the second postulates the absence of definite chemical compounds with stoichiometric relations. Making use of the Donnan equilibrium, as it is called, Procter,[1] Wilson and Wilson,[2] and Jacques Loeb[3] have been very successful in calculating the swelling of gelatine in acids on the explicit assumption that gelatine is an elastic jelly, insoluble in but completely permeable to water, which forms a definite compound with acids, gelatine hydrochloride, for instance.

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[1] J. Chem. Soc., *105,* 313 (1914).

[2] J. Am. Chem. Soc., *40,* 886 (1918).

[3] "Proteins and the Theory of Colloidal Behavior."

Gelatine hydrochloride is also assumed to be soluble in water and completely dissociated; but the gelatine ion will not diffuse through gelatine and consequently the latter acts as a semipermeable membrane to gelatine hydrochloride, though not to chlorine ion. By combining the formula for the Donnan equilibrium with Hooke's law, Wilson gets a formula which reproduces admirably Procter's data on the swelling of gelatine, and he considers this a conclusive proof that gelatine and hydrochloric acid form a definite chemical compound, gelatine hydrochloride.

This is true only in case the formulas obtained by Loeb and Wilson cannot be deduced on some other assumption. Donnan[1] has pointed out that "an adsorption of hydrogen ions by colloidal aggregates or micelles (constituting the units of the 'molecular' network) would lead to the same general equations as the ionization of the amphoteric protein molecules assumed by Procter."

A. V. Hill[2] is equally definite. "It is contended by Loeb that the Donnan Membrane Equilibrium involving the presence of an indiffusible ion is the basis of the colloidal properties of a protein solution. While the possibility of this conclusion is admitted, it is pointed out that one of the chief arguments employed in its favour by Loeb is incorrect. Loeb shows that the potential difference observed experimentally between a protein and a non-protein solution separated by a membrane agrees very exactly with that 'calculated' from the difference in hydrogen-ion concentrations also observed experimentally, and concluded that this supports his theory. As a matter of fact, this equality is a necessary and inevitable consequence of the manner in which his observations were made and of general thermodynamical reasoning, and its proof is independent of any theory of the mechanism by which the potential difference is produced."

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[1] Chem. Reviews, *1*, 87 (1924).

[2] Proc. Roy. Soc., *102A*, 705 (1923).

I do not advocate trying to turn all students either into guessers or into accumulators. A man who is of the accumulator type cannot be converted successfully into the guesser type and it may be a waste of good material to try to do so. It is hard to believe that anything would have kept T. W. Richards, for instance, from making exact measurements, and it would have been a great loss to science if that had happened, because he was the best chemist at exact measurements that has ever lived. When he tried to do something else, as in his work on compressible atoms, the theoretical side was depressing.

There is a passage in Wallace's Russia which has always interested me. "Of all the foreign colonists the Germans are by far the most numerous. The object of the Government in inviting them to settle in the country was that they should till the unoccupied land and thereby increase the national wealth, and that they should at the same time exercise a civilising influence on the Russian peasantry in their vicinity. In this latter respect they have totally failed to fulfil their mission. A Russian village, situated in the midst of German colonies, shows generally, so far as I could observe, no signs of German influence. Each nationality lives *more majorum,* and holds as little communication as possible with the other. The *muzhik* observes carefully—for he is very curious—the mode of life of his more advanced neighbours, but he never thinks of adopting it. He looks upon

Germans almost as being of a different world—as a wonderfully cunning and ingenious people, who have been endowed by Providence with peculiar qualities not possessed by ordinary Orthodox humanity. To him it seems in the nature of things that Germans should live in large, clean, well-built houses, in the same way as it is in the nature of things that birds should build nests; and as it has probably never occurred to a human being to build a nest for himself and his family, so it never occurs to a Russian peasant to build a house on the German model. Germans are Germans, and Russians are Russians—and there is nothing more to be said on the subject."

I do not object at all to people, who should be accumulators, becoming accumulators. When a graduate student tells me that he wishes to make exact measurements, I do not try to show him the error of his ways; I advise him to work with somebody else. I do object however to a student, who might have become a guesser, being forced into the ranks of the accumulators because he does not know that there is another and better type of research, and because he does not appreciate the futility of the slogan "First get your facts."

Only recently I heard of a boy who had made an excellent showing in chemistry as an undergraduate and as a graduate. His nose was kept, however, to the grindstone of exact measurements and that bored him because he was evidently not intended by nature to be an accumulator. He got out of chemistry and we shall never know whether he would have remained a chemist if this lecture had been published earlier.

II

THE MISLEADING EXPERIMENT

WE have seen that most of the important scientific discoveries are made by the guessers and not by the accumulators, by the men whose slogan is "First get your working hypotheses" and not by the men whose slogan is "First get your facts." This is purely empirical, however, and we should like to know why the Baconian method does not work out well. It is not an unreasonable line of attack and why should it be unworkable? The answer is because of the danger inherent in an experiment. Many people think that the appeal to experiment is the last resort, and of course there is some truth in that. On the other hand, all who do research know how difficult it is to make an experiment come out right when we know how it ought to go; and consequently we put very little trust in an experiment which we do for the sake of getting information.

Meldola[1] has put the case very well. "It is only the active worker—the original investigator—who, by personal appeal to Nature through artificially imposed conditions, *i.e.* experiment, or through observation, *i.e.* ready-made phenomena, has come to understand what a *fact* really means in the scientific sense; to realize how laborious is the process of wooing truth and how ambiguous are the answers often given by Nature to his cross-examinations. I have elsewhere recorded a humorous rejoinder by Darwin on one of the very few occasions when it was my never-forgotten privilege to have met him; as this reply bears so closely upon the present topic I will venture to repeat it. I had been dwelling upon this very point of the difficulty of getting Nature to give a definite answer to a simple question, when, with one of those mirthful flashes that occasionally lighted up his features, he retorted: She will tell you a direct lie if she can."

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[1] Gregory: "Discovery," 40 (1923).

According to Sir Francis Darwin,[1] his father carried this point of view so far that "I think he personified each seed as a small demon trying to elude him by getting into the wrong heap, or jumping away altogether; and this gave to the work the excitement of a game."

I am defending the thesis tonight that an experiment should never be trusted until it has been checked by a good theory. Many people take the opposite view; but I intend to show that they are wrong.

The most extreme people that I know about are Wrinch and Jeffreys.[2] "We notice that where a simple law exists it is obtained from the observations; then it is found that the observations do not fit the law exactly. The discrepancies are called errors and are usually ignored afterwards. This is so far conventional, but nevertheless the convention is an extremely important one. The use of the word 'error' has led to the idea that there is something wrong with such observations, which is not the case. The observation is always right, in consequence of the mere fact that it is an observation. All errors of observations seem to be reducible to this conventional form: a physical law is chosen to fit the observations as closely as possible, giving, in other words, a first approximation; then another law is discovered to be relevant, and the two together give a closer approximation, and so on, the error at each stage being merely the unexplained balance. Thus an error is just the difference between an observed quantity and its value predicted by the combination of all the physical laws so far known to be relevant. The 'theory of errors' appears to be an extraneous hypothesis, based on a particular plausible assumption about the nature of errors; its utility is chiefly in giving a unique solution in particular cases where the solutions that fit the facts almost equally well differ considerably among themselves. This advantage is, however, liable to be exaggerated."

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[1] Henshaw Ward: "Charles Darwin," 380 (1927).

[2] Phil. Mag. (6) *42,* 377 (1921).

I doubt whether many scientific people would admit that all errors of observation are reducible to this conventional form, because then there would be no errors of observation until one had a theory. I am certain that practically no scientific people would accept the bald statement that "the observation is always right in consequence of the mere fact that it is an observation." I doubt whether even Wrinch and Jeffreys would stand for this statement when detached from the context.

The authors have also overlooked one important distinction between the orthodox physical chemist and the followers of Mother Eddy. The Christian Science people deny the existence of sickness but recognize error. The orthodox physical chemist of today recognizes sickness but denies the existence of error. His universal panacea, the activity concept, enables him to fit a set of bad experiments absolutely to a bad theory or to any number of bad theories, though not simultaneously.

Some of the psychologists push Wrinch and Jeffreys pretty hard. Everybody knows, as an abstract proposition, that making one thousand observations instead of one may cut down the probable error due to accidental errors; but that it has no effect on systematic errors. When you come to any particular case, many psychologists believe that they will get the right answer if they make enough experiments. At one time Cattell was interested in determining, among other things, what colors scientific men liked best. When I took the test I found that all his colors except the yellow were bad. It was like trying to find out which one of six meats a man liked best and smearing five of them with garlic. There might be enough people from the south of France to keep the test from being unanimous, in which case one could calculate a probable error. Cattell was not interested at all in the question whether his test was a good one or not. If he could get enough data, he felt sure that the answer would be right. Anybody but a professional psychologist would have been a bit worried over a method of comparison[1] which does not put either the Department of Chemistry of the University of California or the Department of Physics of Princeton University among the first ten in its respective field. I should class that as a striking case of the misleading experiment.

The phenomenological psychologists seem to be pure Baconians. They pride themselves on not having a working hypothesis and on making statistical studies of what they observe, believing that in that way they will acquire merit—which may be true—and will attain to truth—which they certainly will not do. I am picking out a paper by Bixby,[2] entitled "A Phenomenological Study of Luster," because I once had to study the theory of metallic luster in its bearing on the colors of colloids. When considering metallic luster, no experiments were made with dyes showing selective reflection or with oil films on a dark background. Silver, copper, lead, galena, exfoliated mica, and an Indian pheasant's head showed metallic luster. "The characteristic feature here is large, smooth, almost glass-like reflection." Here the author is confusing specular reflection and luster. The metallic colors on the grackle are differentiated from those on the Impeyan pheasant, the first being called submetallic. Since both are thin-film colors, they must be alike unless the thin films are farther down beneath the surface in the case of the grackle; but there is nothing to show that. The exfoliated mica is called metallic and the colored gelatine films not. They are two cases of the same thing and must be classified together, unless the difference in the surface has an effect. As against that, Bixby has ruled out the effect of surface. Bixby admits that the synthetic pie plate "looks like an aluminum pie plate;"[1] but he is sure that there is no metallic luster. It does not appear whether this is because aluminum does not have metallic luster; and no experiments were made with a rotating disc which should look like copper, which he does consider to have a metallic luster.

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[1] Cattell: "American Men of Science," 1128 (1927).

[2] J. Gen. Psychology, *1*, 136 (1928).

What it comes down to is that the phenomenological psychologist makes a non-critical and superficial series of observations, and is quite indifferent whether he separates cases which should belong together or not. For this reason it is impossible for him to get at basic principles except by accident.

There is one case of a misleading experiment which has been tried by inconceivable millions of people, each many thousands of times. It always comes out wrong and no amount of knowledge ever makes it come out right. No matter how hard we try, we always see the sun go round the earth instead of seeing the earth revolving with the sun relatively stationary.

Newton was once the victim of the misleading experiment.[1] "Taking the value accepted in 1666 [for the radius of the earth], Newton calculated that the moon would fall towards the earth by 44 thousandths of an inch in a second; in other words, this is the amount by which the path of the moon should deviate from a straight line in a second in order to describe its curved path around the earth in a month. The amount by which the moon actually swerves from a rectilinear path in a second is 53 thousandths of an inch, so Newton saw that the calculated result did not agree with what was then considered to be a fact. The discrepancy between theory and observation "induced him to abandon the subject and pursue other studies with which he had been previously occupied."

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[1] Bancroft and Allen: J. Phys. Chem., *29,* 581 (1925).

"Not until 1684 did he 'resume his thoughts concerning the moon.' A few years before that time an accurate determination of the size of the earth had been made by Picard, and this gave a more correct value of the length of the earth's radius than had previously been accepted. Using the new value, Newton again took up the calculation of the moon's deflection from a straight path, on the assumption of the force controlling it being the same as that which causes bodies to fall on the earth, but diminished in strength on account of the distance of the moon from the earth. The result now obtained showed that the central force should cause the moon to swerve from a straight-line path by 53 thousandths of an inch in a second, which is exactly the amount that will produce the curved path described by the moon around the earth."

Most of us would have considered 44 and 53 as of the same order of magnitude. If Newton had had the courage to say that his reasoning was right and that the experiment must be wrong, he would have saved nearly twenty years.

[1] Whewell: "Novum Organum Renovatum," *81* (1858); Gregory: "Discovery," 171 (1923).

About the middle of the eighteenth century Needham[1] sent a paper to the Royal Society in which he described heating mutton gravy, putting it in a bottle which he closed with a cork to prevent anything coming in from outside, heating the bottle again, and then putting it away. At the end of a few days the liquid was swarming with animalcules.[2] The same result could be obtained with a soup made from seeds or almonds. This looks like a clear-cut proof of spontaneous generation; but the Abbé Spallanzani was not convinced. He had an innate fear of the misleading experiment. He trusted his general theory against the experimental evidence and he started in to find the flaw in Needham's work. He thought of two possible sources of error. Needham might not have stoppered his bottles well enough or he might not have heated them long enough to destroy all life. The first possibility could be eliminated by sealing the glass necks in a flame. The second could be tested by heating the sealed flasks for different lengths of time. The tests were conclusive. The bottles which had been boiled for a few minutes showed animalcules; those which had been boiled for an hour did not. This proved that there were organisms which were killed by long boiling and not by short boiling. When once killed, there was no spontaneous generation of life. Spallanzani was a good experimenter, so he repeated Needham's experiments, using corks but heating the flasks for different lengths of time. All the flasks contained animalcules, which showed that closing with corks was not adequate.

Galvani assumed that the twitching of a frog's legs when the muscles and nerves were touched simultaneously by wires was because the frog's legs acted like a Leyden jar, the muscles corresponding to one coating and the nerves to the other. If this were true, the twitching should be greater if one coated the muscles and the nerves with tin-foil, just as one puts tin-foil on the inside and the outside of a Leyden jar. The experiment confirmed this prediction; but was misleading because Volta showed that the same effect could be obtained by touching the nerve at two points. The frog proved to be an electroscope and not a Leyden jar.

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[1] Phil. Trans., *45,* 615.

[2] DeKruif: "The Microbe Hunters," 31 (1926).

In 1802 it was already known that water was composed of hydrogen and oxygen. It therefore occurred to Simon, in Berlin, that the weight of the hydrogen and oxygen obtained by electrolysis of water should be exactly equal to the weight of water during the electrolysis.[1] The first experiment showed a weight of 1.56 grains of oxyhydrogen gas and a loss of water equal in weight to 2.2 grains. The grain is equal to 0.0648 grams. Instead of drawing the conclusion that water is equal to hydrogen and oxygen plus about forty per cent by weight of electricity, Simon said to himself that the theory is right and the experiment is wrong. On thinking over the possible sources of error, it seemed probable that water vapor had been carried off with the oxyhydrogen gas. A second run, made with all precautions, lasted seventy-two days, at the end of which time he had collected 4.61 grains of oxyhydrogen gas and had lost 4.60 grains of water, equivalent to about 0.3 grams. The two results were now equal within the limits of experimental error. Purely as a side issue, it is interesting to note that it took Simon seventy-two days of continuous electrolysis to decompose three-tenths of a gram of water.

The work of Sir Humphry Davy[2] on the electrolysis of water is one of the classical cases of the struggles between a skillful experimenter and recalcitrant experiments. In

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[1] Ostwald: "Elektrochemie," 164 (1896).

[2] "Works of Sir Humphry Davy," *5*, 2 (1840).

1800 Nicholson and Carlisle had electrolyzed the so-called pure water of the day, getting hydrogen at one pole and oxygen at the other, which was as it should be. In addition there was a base formed at the hydrogen pole and an acid at the oxygen pole. Davy felt certain that the formation of a base and an acid in addition to the evolution of hydrogen and oxygen was the result of experimental error.

The first experiments were made with platinum electrodes and distilled water in agate cups connected by fibrous asbestos. Both asbestos and the agate cups had been boiled for some hours in distilled water. Some hydrochloric acid was found at the oxygen pole and some soda at the hydrogen pole. "This result was unexpected, but it was far from convincing me that the substances which I had obtained were generated."

Thinking that the agate might contain traces of salt, the electrolysis was repeated again and again with fresh samples of water. The hydrochloric acid disappeared in time and most of the soda; but there was nitrous acid at the oxygen pole and traces of fixed alkali at the hydrogen pole. The electrolysis was repeated with vessels of pure gold instead of agate ones. The amount of fixed alkali was decreased; but there was quite a good deal of nitrous acid, relatively speaking, at the oxygen pole.

"It was now impossible to doubt that the water contained some substance in very minute quantities, capable of causing the appearance of fixed alkali, but which was soon exhausted; and the question that immediately presented itself was, Is this substance saline matter carried over in distillation? Or is it nitrogen gas which exists in minute portions in all water that has been exposed to air, and which, if an element of the fixed alkali, would under the circumstances of the experiment have been soon exhausted, whilst its absorption from the atmosphere would be impeded by the saturation of the water with hydrogen?

"I was much more inclined to the former than to the latter supposition. I evaporated a quart of the distilled water that I had used, very slowly at a heat below 140° Fahrenheit, in a silver still; a solid matter remained equal to 7/10 of a grain [about 45 mg]; this matter had a saline but metallic taste, and was deliquescent when exposed to air: I could not obtain from it regular crystals; it did not affect turmeric or litmus, but a part of it, after being heated red in a silver crucible, exhibited strong alkaline properties. It was not possible to make a minute analysis of so small a quantity, but it appeared to me to be principally a mixture of nitrate of soda and nitrate of lead; and the metallic substance, it is most likely, was furnished by the condensing tube of the common still.

"The existence of saline matter in the distilled water being thus distinct, it was easy to determine its operation in the experiment. I filled the two gold cones with water in the usual manner; that negatively electrified, soon attained the maximum of its effect upon turmeric paper. I then introduced into it a very minute portion of the substance obtained by the process of evaporation that has been just described; in less than two minutes its effects were evident; and in five minutes the tint of the paper was changed to a bright brown.

"I now conceived that by collecting the water obtained in the second process of slow distillation I should be able to carry on the experiment without *any* appearance of fixed alkali, and the trial proved that I was not mistaken."

"I had never made any experiments in which acid matter having the properties of nitrous acid was not produced, and the longer the operation the greater was the quantity that appeared.

"Volatile alkali likewise seemed to be always formed in very minute portions, during the first few minutes in the purified water in the gold cones, but the limit to its quantity was soon attained.

"It was natural to account for both these appearances, from the combination of nascent oxygen and hydrogen respectively with the nitrogen of the common air dissolved in water: and Dr. Priestley's experiments on the absorption of gases by water (on this idea) would furnish an easy explanation of the causes of the constant production of the acid, and the limited production of the alkali: for hydrogen, during its solution in water, seems to expel nitrogen; whilst nitrogen and oxygen are capable of co-existing dissolved in that fluid.

"To render the investigation more complete, I introduced the two cones of gold with purified water under the receiver of an air-pump; the receiver was exhausted till it contained only 1/64 of the original quantity of air; and then, by means of a convenient apparatus, the tubes were connected with an active Voltaic pile of 50 pairs of plates of 4 inches square. The process was carried on for 18 hours, when the result was examined. The water in the negative tube produced no effect upon prepared litmus, but that in the positive tube gave it a barely perceptible tinge of red.

"An incomparably greater quantity of acid would have been formed in a similar time in the atmosphere, and the small portion of nitrogen gas remaining in contact with the water, seemed adequate to the effect.

"I repeated the experiment under more conclusive circumstances. I arranged the apparatus as before; I exhausted the receiver, and filled it with hydrogen gas from a convenient airholder; I made a second exhaustion, and again introduced hydrogen that had been carefully prepared. The process was conducted for 24 hours, and at the end of this time neither of the portions of the water altered in the slightest degree the tint of litmus.

"It seems evident then that water, chemically pure, is decomposed by electricity into a gaseous matter alone, into oxygen and hydrogen."

These experiments are usually cited to show Davy's ingenuity in detecting the flaws in what seemed like perfect experiments, and they do illustrate this admirably. Even more important is that Davy was always convinced of the accuracy of his working hypothesis and that he never wavered from the belief that every discrepancy between theory and experiment was due to experimental error.

Wedgewood and Davy studied the action of light on paper or white leather impregnated with silver nitrate and were able to obtain photographs which seemed good to them. The matter was dropped because they could find no way of removing the unreduced silver salt. Since silver nitrate is very soluble in water, they thought that it should be easy to wash it out of paper; but they did not know about adsorption and consequently they drew the false conclusion that there could be no silver nitrate in the paper.

In December 1824 Faraday attempted to obtain an electric current by means of a magnet, and on three occasions he had made elaborate but unsuccessful attempts to produce a current in one wire by means of a current in another wire or by a magnet. It was not until August 1831, nearly seven years later, that he succeeded in making the experiment fit the theory. He had even more trouble with the electromagnetic rotation of the plane of polarized light, for it took him more than twenty years to get a positive result. People say that this long series of researches is an instance of his perseverance; but there would have been no sense in persevering if he had been wrong. In that case he would have been called pig-headed. The important thing was that he knew all the time that the negative results were misleading and that the theory was better than the experiments.

In 1851 William Thomson[1] (afterwards Lord Kelvin) published a paper in which he calculated the electromotive force of a voltaic cell from the thermochemical data, on the assumption that the cell could be so arranged that the only heat evolved was the Joule heat and therefore proportional to the square of the current. He tested the equation on the Daniell cell, which consists of $Zn|ZnSO_4|CuSO_4|Cu$, and found a perfect agreement. The equation, $I = \theta$, or $E = Q$ in modern units—has been known since then as Thomson's law. It was the most natural thing in the world to take the Daniell cell. It was practically the only constant cell in use at the time and it came pretty near to being a completely reversible cell. There was no reason to suppose that there was anything abnormal about it. As it happens, however, Thomson's law holds only when the temperature coefficient of the electromotive force is zero; and the Daniell cell is one of a very few which has practically a zero coefficient. Thomson was fooled by the misleading experiment, and Helmholtz was the man who deduced the exact formula about thirty years later.

When Cailletet and Pictet first liquefied hydrogen by releasing the pressure, they reported that the escaping jet of liquid had a steel blue color.[2] This did not seem surprising in 1878 because people rather expected that solid hydrogen would be a metal. We know now that liquid hydrogen is colorless and that the jet cannot have shown a pigment blue. It is another case of the misleading experiment. The liquid drops in the hydrogen mist were of such a size and the light was in such a place, that the observer saw a Tyndall blue, a structural color. It cannot have been anything else and yet the statement about the blue hydrogen appears in many books to this day.

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[1] Phil. Mag., (4) *2,* 429 (1851).

[2] "Collected Papers of Sir James Dewar," *1,* 169 (1927).

In 1873 Dewar[1] was caught for some years by the misleading experiment. He determined the vapor density of potassium in a cylindrical iron bottle of at least 200 cc capacity. "In the experiments we have hitherto carried out, we have satisfied ourselves that the amount of mercury vapour *not* swept out by the potassium is quite inappreciable; and as our object has been in the meantime merely to arrive at approximate results and to perfect our methods of manipulation, we have neglected the minute correction, which, on account of that small remnant of mercury, ought, strictly speaking, to have been applied to the volume of the vapour as calculated from the capacity of the bottle in the cold, the coefficient of expansion of iron, and the temperature (1040° Deville) at which the vapour was measured. The results of our observations show conclusively that the density of potassium-vapour, as produced in the process described, cannot exceed 45 times that of hydrogen, and that therefore the molecule of potassium consists of *two atoms* (K_2)."

In 1879 another run[2] was made by the recently-discovered Victor Meyer method, using wrought iron bottles. The molecular weights thus found for potassium varied from 74.4 to 90.5, with a mean value of 81.6, which corresponds closely enough to a diatomic vapor. The authors say that "the above experiments show that no great accuracy in the determination of the vapour-density of sodium and potassium can be obtained by this method of working when vessels of wrought iron are employed. The results are, however, conclusive as regards the *normal character* of *their respective vapour-densities*."

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[1] Proc. Roy. Soc., *21,* 203 (1873).

[2] Dewar and Scott: Proc. Roy Soc., *29,* 206 (1879).

Only a little later these conclusions went by the board completely. Experiments were made[1] in a platinum vessel packed in magnesia instead of sand. "The new determinations of the molecular weights of potassium and sodium are just about half the former values, and would seem to support the inference that the atom of each of these metals resembles mercury and cadmium in the gaseous state, as regards molecular volume. Such a remarkable result cannot be accepted without a very thorough investigation of the secondary reactions which may be induced at high temperatures. It is certain that in the platinum vessels there is much less absorption of vapour than in the case of iron, and that the action of the furnace gases is all but eliminated. The following suggestions might be made to explain the results. In the first place, the metals may contain a large amount of occluded hydrogen. With reference to this assumption, we have examined the sodium used in our experiments and find, on careful exhaustion and boiling in a Sprengel vacuum, that one volume of sodium only contains one-third of its volume of occluded hydrogen, so that this will not account "for the double volume of the atom of the metals." The alkali metals may form volatile compounds with platinum, although, judging from analogy, such compounds are not likely to exist. If so, the vapour blown out of the bottle ought to contain platinum. The use of platinum tubes to blow out the vapours prevented the test being applied. The metals may be changed into oxides from occluded oxygen in the platinum. If so, no free metal ought to remain in the bottle after this rapid evaporation. This however is not the case; so that, unless some other explanation can be given, it will be necessary to admit some anomaly in the behaviour of potassium and sodium, as regards their vapour-densities at high temperatures. In future the variation of the density with the temperature must be determined, but this is very difficult at temperatures about 1500° C. The chief drawback to the prosecution of the experiments is the terrible waste of platinum vessels, which never stand the combined action of potassium, sodium, and hydrogen temperatures beyond two or three operations."

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[1] Ibid., 490 (1879).

Von Babo[1] and Wüllner[2] studied the change of the vapor pressures of aqueous salt solutions with temperature and concentration. Von Babo found that, for a given concentration, the percentage lowering of the vapor pressure was pretty nearly independent of the temperature. Wüllner found that, at constant temperature, the percentage lowering of the vapor pressure was pretty nearly proportional to the concentration. It was perfectly natural that they should have worked with aqueous salt solutions. Water is the cheapest of all solvents and can be obtained more nearly pure than any other solvent. Salts are the cheapest of all solutes and are more easy to purify than any other solute. By the irony of fate, the most natural combination to have studied was the worst possible. As we know now, organic solutes in organic solvents are normal as a rule. Organic solutes in water are normal and salts in alcohol are usually normal. It is only when we come to salts in aqueous solution that we meet the difficulties of electrolytic dissociation. By avoiding this pitfall Raoult carried off all the honors.

One wonders whether Debye and Hückel may not have made a similar mistake by starting with salts in aqueous solution. They make the explicit assumption that all the variations are due to the interaction of the ions and they adjust their ionic diameters to fit their assumption. They have not proved that there are no other disturbing factors and the presumption is strongly against them. It might have been wiser to have cleaned up the case of organic solutes in organic solvents before tackling the job that really interested them. Some day people will point to the work of Debye and Hückel as an extraordinarily interesting case of the misleading experiment.

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[1] Ostwald: "Lehrbuch allgem. Chem.," *1,* 706 (1891).

[2] Pogg. Ann., *103,* 529; *105,* 85 (1858); *110,* 564 (1860).

Pfeffer found osmotic pressures of about 52 cm for a one per cent solution of cane sugar with a copper ferrocyanide membrane and pressures of about 36-39 cm when membranes of calcium phosphate and Prussian blue were used. From this he concluded that the osmotic pressure was a function of the nature of the membrane. Since there is always an apparent effect of the nature of the membrane when one measures osmotic pressures experimentally, it is not surprising that the people who believe in the trustworthiness of an experiment, should continue to hold this belief.[1] On the other hand the use of the chemical potential by Gibbs[2] or the method of van't Hoff is absolutely convincing that the magnitude of the osmotic pressure cannot be a function of the nature of the membrane. In a letter to me, written years ago, Raoult expressed his doubts whether there was, experimentally, such a thing as a semipermeable membrane. In this work with ether and methyl alcohol using diaphragms of rubber and of pig's bladder, Raoult[3] had satisfied himself that he was only measuring the difference between two flows in opposite direction.

[1] Cf. Kahlenberg: J. Phys. Chem., *10,* 141 (1906).

[2] "The Scientific Papers of J. Willard Gibbs," *1,* 83 (1906).

[3] Z. physik. Chem., *17,* 737 (1895).

Raoult was probably right in this. If we have a non-porous membrane, like rubber, through which one liquid passes because it is soluble in the membrane, the presence of that liquid in the membrane will cause the second liquid to dissolve to some extent in the membrane. Consequently, the membrane is no longer theoretically semipermeable even though it may have been theoretically impermeable to the second liquid in the absence of the first. If we have a porous membrane, like copper ferrocyanide, which owes its semipermeability to strong negative adsorption, we can hardly say that the concentration of the solute in the pores will be absolutely zero for any concentration of the solution, and it will certainly not be zero for high concentrations of the solute in the solution. We can calculate osmotic pressures more accurately than we can measure them. If Pfeffer, Kahlenberg, and others had said that the maximum osmotic pressure actually will practically always depend on the nature of the membrane, nobody could have disputed that. They did not know about the misleading experiment and they made the entirely warranted assumption that measurements had been made with membranes which were theoretically semipermeable under the conditions of the experiment.

If we start with the law of the conservation of energy and Ohm's law, we can deduce Joule's law that the heat developed in a metallic conductor is proportional to the first power of the resistance and to the second power of the current. Experiment failed at first to confirm this relation, especially for higher currents. The cause of the misleading result was the heating of the wire, which changed the resistance.

Raoult's first work[1] on the vapor pressures of solutions established a relation between the lowering of the freezing-point and the lowering of the vapor pressure at 100° for the same aqueous solutions. He found the beautifully simple relation $7.6\Delta = p_0 - p_1$, where Δ is the lowering of the freezing-point and $p_0 - p_1$ the lowering of the vapor pressure. There was no reason to suppose that there was anything abnormal about water and consequently no reason to suppose that the relation was not general. As a matter of fact, Raoult had started, quite unconsciously, with the only liquid, so far as we now know, which would have given him this relation. On the theory of probability this is practically impossible; but it is just what one would expect from the misleading experiment.

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[1] Compt. rend., *87*, 167 (1878).

The real formula for the relation between the lowering of the freezing-point and the lowering of the vapor pressure at 100°, if we assume von Babo's law, is

$$\Delta = 18.5\frac{100}{M_2}.\frac{p_0 - p_1}{p_0}$$

where Δ is the freezing-point lowering, M_2 is the molecular weight of the solvent as vapor, and 18.5 is the freezing-point constant. For water M_2 is 18 and $p_0 = 760$ at 100°. The equation therefore becomes[1] $7.6\Delta = \frac{18.5}{18}(p_0 - p_1)$, which is Raoult's relation within his limits of experimental error. It was pure coincidence that the freezing-point constant for water has very nearly the same numerical value as the gram molecular weight of water vapor. For benzene the corresponding ratio is a little over 0.6 instead of being practically unity as in the case of water.

Raoult[2] had other difficulties with the misleading experiment. He found experimentally that the molecular lowering of the freezing-point for one gram-molecular weight of the solute in 100 gram-molecular weights of the solvent came out approximately as follows: formic acid, 0.63°; acetic acid, 0.65°; benzene, 0.64°; nitrobenzene, 0.59°; ethylene dibromide, 0.63°; water, 2.61°. With the exception of water, the values of all the solvents tested (five) varied between 0.59° and 0.64° with a mean value of 0.63°. "Water is the only exception; but that is not surprising for a liquid which shows many other peculiarities and it is permissible to explain the anomaly by assuming that each of the physical molecules, of which it is formed, consists of several chemical molecules united together. At a time when I had only obtained a small number of molecular lowerings higher than the mean lowering of 37 [maximum molecular lowering for one mol per hundred grams of solvent] in aqueous solutions, I believed that the physical molecules of water were composed of three chemical ones. In fact, if we divide 37 by 18×3, we get as quotient 0.685, which is not very far from the mean quotient of 0.63 obtained with the other solvents. I have called attention to this in an earlier paper.[1] But new determinations make it no longer possible to consider the molecular lowerings distinctly higher than 37 as doubtful. I am, therefore, compelled to recognize that molecular lowerings in water may really rise to 47, a maximum value in many cases where the dissolved substances do not decompose. To explain such a lowering, *it is sufficient to assume that the molecules of water are united in fours,* at least near zero. The quotient of 47 by 18×4, is 0.65, a number remarkably close to the mean value of 0.63 obtained with the other solvents."

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[1] Through an oversight p_0 has been left in the denominator in the paper by Bancroft and Davis: J. Phys. Chem. *32,* 30 (1928).

[2] Compt. rend., *101,* 1056 (1885).

It must have given Raoult a real thrill to have brought all six solvents together like this. We know now that it is not justifiable to take 47 for the molecular lowering, and we know now that the molecular weight of liquid water has nothing to do directly with the lowering of the freezing-point; but Raoult did not know this. Leaving water out of account, it seems perfectly impossible that all the other five solvents should have come together as they did purely by coincidence, and yet they did. We know now that this particular relation does not hold and that the heat of fusion is the important factor in determining the lowering of the freezing-points. I do not criticize Raoult at all for making this mistake. I feel that he was the victim of fate. His two big generalizations were correct, and he slipped up on two minor ones, which nobody in the world except van't Hoff could have avoided. Raoult's experience should put each of us on his guard against the danger of the misleading experiment. As Darwin said: "Nature will tell you a direct lie if she can."

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[1] Compt. rend., *95,* 1030 (1882).

Roux's work on the diphtheria bacillus[1] is a striking case of the triumph of conviction—one can hardly say theory—over recalcitrant experiments.

"Roux and Yersin went to the Hospital for Sick Children—diphtheria was playing hell with Paris—and here they ran on to the same bacillus Loeffler had found. They grew this microbe in flasks of broth, and did the regular accepted thing first, shooting great quantities of this soup into an assorted menagerie of unfortunate birds and quadrupeds who had to die without the satisfaction of knowing they were martyrs. It wasn't particularly enlightened searching, this, but almost from the tap of the gong, they stumbled on one of the proofs Loeffler had failed to find. Their diphtheria soup paralyzed rabbits! The stuff went into their veins; in a few days the delighted experimenters watched these beasts drag their hind legs limply after them; the palsy crept up their bodies to their front legs and shoulders —they died in a clammy, dreadful paralysis. . . ."

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[1] DeKruif: "The Microbe Hunters," 188-192 (1926).

It seemed certain to Roux that this bacillus was the cause of diphtheria in human beings; but he could not find any trace of the bacilli in the tissues of the dead rabbits. Then Loeffler's prediction flashed over Roux. "It must be that the germs make a poison in this broth to paralyze and kill the beasts . . ." he pondered.

"He took big glass bottles and put pure microbeless soup into them, and sowed pure cultivations of the diphtheria bacillus in this broth; into the incubating ovens went the large-bellied bottles. At the end of four days they filtered the soup through a bougie. The filtrate was injected into rabbits and guinea-pigs with no result. The animals did not die."

In spite of all the evidence to the contrary, Roux said: "There must be a poison in this soup where the diphtheria germs have grown—else why should those rabbits have died?"

"Then—I have told scientific searchers about this and they have held their noses at such an experiment—Roux nearly drowned a guinea-pig. For weeks he had been injecting more and more of his filtered soup, but now (it was like facing a night on a park bench with your last dime on the two dice) he injected thirty times as much! Not even Pasteur would have risked such an outlandish dose—thirty-five cubic centimetera Roux shot under the guinea-pig's skin and you would expect that much water would kill such a little beast. If he died it would mean nothing. . . . But into the belly of a guinea-pig and into the ear-vein of a rabbit went this ocean of filtered juice—it was as if he had put a bucketful of it into the veins of a middle-sized man.

"But that was the way Roux carved his name on those tablets which men while they are on earth must never allow to crumble; for, though the rabbit and the guinea-pig stood the mere bulk of the microbe-less broth very well, and appeared perfectly chipper for a day or so afterwards, in forty-eight hours their hair was on end, their breath began to come in little hiccups. In five days they were dead, with exactly those symptoms their brothers had, after injections of the living diphtheria bacilli. So it was Emile Roux discovered the diphtheria poison. . . .

"By itself this weird experiment of the gigantic dose of feebly poisonous soup would only have made microbe hunters laugh. It was scandalous. 'What!—if a great flask of diphtheria microbes can make so little poison that it takes a good part of a bottle of it to kill a small guinea-pig—how can a few microbes in a child's throat make enough to do that child to death? It is idiotic!'

"But Roux had got his start. With this silly experiment as an uncertain flashlight, he went tripping and stumbling through the thickets, he bent his sallow bearded face (sometimes it was like the face of some unearthly bird of prey) over a precise long series of tests. Then suddenly he was out in the open. Presently, it was not more than two months later, he hit on the reason his poison had been so weak before—he simply hadn't left his germ-filled bottles in the incubator for long enough; there hadn't been time enough for them really to get down to work to make their deadly stuff. So, instead of four days, he left the microbes stewing at body temperature in their soup for forty-two days, and when he ran that brew through the filter—presto! With bright eyes he watched unbelievably tiny amounts of it do dreadful things to his animals—he couldn't seem to cut down the dose to an amount small enough to keep it from doing sad damage to his guinea-pigs. Exultant he watched feeble drops of it do away with rabbits, murder sheep, lay large dogs low. He played with this fatal fluid; he dried it; he tried to get at the chemistry of it (but failed) ; he got out a very concentrated essence of it though, and weighed it and made long calculations.

"One ounce of that purified stuff was enough to kill six hundred thousand guinea-pigs—or seventy-five thousand large dogs! And the bodies of those guinea-pigs who had got a six hundred thousandth of an ounce of this pure toxin—the tissues of those bodies looked like the sad tissues of a baby dead with diphtheria. . . ."

In 1817 Bunsen tried to determine the rate of explosion of an ignited gas mixture[1] and he went about it in what seemed like a very ingenious way. "An explosive mixture of gases was sent through a small orifice in the end of a tube, and the jet was ignited. In the Bunsen burner, it will be remembered, the flame is only steady when the mixture of gases travels along the tube faster than the flame can travel through the combustible gases. If otherwise, the flame 'strikes back.' Now, Bunsen measured the least speed which would prevent the flame striking back into the reservoir. In this way Bunsen found that a mixture of two volumes of hydrogen and one volume of oxygen burns at the rate of about 34 metres per second, while most other gases burn at the rate of about a metre per second."

It seemed reasonable to suppose, as Bunsen did, that, subject to minor corrections, "when the velocity of efflux of the gas is equal to the velocity of propagation of explosion the flame will not run back." Bunsen's results stood for fourteen years; but the misleading experiment had fooled him after all. Berthelot and Vieille and Dixon measured the actual detonation wave in tubes and found a velocity of about 2800 meters per second for oxyhydrogen gas as against the thirty-four meters found by Bunsen. The two rates are not of the same order of magnitude.

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[1] Mellor: "Chemical Statics and Dynamics," 450 (1904).

In Cohen's doctor dissertation[1] on the electrical determination of inversion points, the temperature coefficient for a cell with hydrated and anhydrous sodium sulphate was calculated to be about 1.1 millivolt per degree between 28.3° and 32.6°. The experimentally found values were about 8-9 millivolts, a hopeless disagreement. Cohen had the courage of his convictions for he said: "I believe that these differences are due to overlooking some factor, whose nature is for the moment a complete mystery."

A little later he had his reward[2], for it was found that some of the samples of sodium sulphate were alkaline to phenolphthalein while others were acid to methyl orange. Working with carefully neutralized sodium sulphate the calculated temperature coefficient was 1.2 millivolts as against an experimentally observed value of 1.5 millivolts.

Nichols and Franklin[3] thought they had proved that a strong magnetic field destroyed the passivity of iron in nitric acid, sp. g. 1.40 "When finely divided iron is placed in a magnetic field of considerable intensity and exposed to the action of an acid, the chemical reaction differs in several respects from that which occurs under ordinary circumstances. The cause of one such difference may be found in the fact that the solution of iron in the magnetic field is in a sense equivalent to its withdrawal by mechanical means to an infinite distance. Mechanical removal requires the expenditure of work and the same thing is doubtless true of what might be called its chemical removal. In other words the number of units of heat produced by the chemical reaction should differ, within and without the field, by an amount equivalent to the work necessary to withdraw the iron to a position of zero potential. Experiments upon this point which have been briefly described in a note already published in abstract,[1] brought out other and unlooked-for modifications of the reactions."

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[1] Z. physik. Chem., *14,* 91 (1894).

[2] Cohen and Bredig: Z. physik. Chem., *14,* 545 (1894).

[3] Am. J. Sci., (3) *31,* 272 (1886); *34,* 419 (1887); *35,* 296 (1888).

"Nitric acid was mixed in various proportions with water for the purpose of finding the degree of dilution at which the most rapid, uniform, and complete solution of the powdered iron could be obtained. During the search for the most desirable mixture, a new and very interesting effect of the magnet was accidentally discovered. Five grams of powdered iron lay in the beaker close above the poles of the electro magnet, which was in circuit. Some cold nitric acid was poured upon the iron but the latter remained passive. Wishing to note the character of the reaction, we warmed the beaker slightly, then placed it upon the poles of the magnet and put a thermometer into the solution to get its temperature. The bulb of the thermometer touched the iron in process of stirring the acid, when the hitherto passive mixture burst, almost explosively, into effervescence, and a cloud of red nitrous fumes was given off. The removal of the solution from the field of the magnet restored the passivity of the iron, and the action ceased entirely in a few seconds. All attempts to destroy the passivity by further stirring failed until the beaker was brought back into the neighborhood of the magnet, when, at the merest touch with the thermometer, or any glass rod, the reaction would begin again and continue until the beaker was removed from the field. It was found possible to render the iron passive and to destroy that passivity at will by the above process; and after the solution had become warm the reaction was found to begin spontaneously whenever the beaker was brought into the field."

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[1] A Preliminary Note on the Action of Acids on Iron in the Magnetic Field: Proceedings Am. Association for the Advancement of Science, 1884.

"It appears from these experiments that the action of the magnet is to lower the temperature of transition to the active state, and that the intensity of the magnetic field necessary to convert passive into active iron at a given temperature increases rapidly with the concentration of the acid."

One can hardly believe that this is not a real phenomenon. Everything happens just as Nichols and Franklin say and yet none of it is really so. These results stood for nearly thirty years, until Bennett and Burnham[1] repeated them and found the hidden flaw. "Nichols and Franklin used iron filings, and we have shown that if filings are made passive and placed between the poles of an electro-magnet, and the current turned on, the filings become active. This is due to one piece scratching another, caused during orientation of the particles of iron when magnetized. When a solid piece of iron is passivated in 1.40 nitric acid, and placed in the field, it does not become active."

Since nitric oxide is formed when copper or mercury corrodes chemically in nitric acid and since the electrolytic reduction of nitric acid gives ammonia at a copper cathode and hydroxylamine at a mercury cathode, Tafel[2] denied absolutely any identity between chemical and electrolytic reactions. His reasoning seemed absolutely conclusive; but he was dealing with the misleading experiment, as was shown by Turrentine.[3] When copper reacts chemically with nitric acid, the anode product, copper nitrate, is formed at the same spot where the reduction takes place. In the electrolytic reduction of nitric acid with a copper cathode, the reduction takes place in a solution practically free from copper salt. The conditions are therefore not the same in the two cases. This is the point that was overlooked by Tafel and which vitiates all his conclusions with regard to copper. To show that this difference in conditions was the cause of the difference in the products, Turrentine electrolyzed a solution of copper nitrate and nitric acid, using a copper cathode. A gas was evolved at the cathode, which proved on analysis to be chiefly nitric oxide. This experiment can be done in another way which is more striking. If dilute nitric acid be electrolyzed between copper electrodes, there will be at first no evolution of gas at the cathode; but gas will begin to appear as soon as the blue solution formed at the anode comes in contact with the cathode.

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[1] J. Phys. Chem., *21,* 140 (1917).

[2] Z. anorg. Chem., *31,* 291 (1902).

[3] J. Phys. Chem. *10,* 715 (1906).

Another case of apparent contradiction between the chemical and the electrolytic reactions—which was cleared up by Joss[1]—is in the quantitative, electrolytic determination of nitric acid as ammonia. "Luckow,[2] Vortmann,[3] Ulsch,[4] and Easton[5] all developed methods for the estimate of nitrates by electrolytic reduction to ammonia. A feature of all of the methods is the presence of copper sulphate and sulphuric acid in their solutions. This is obviously the condition under which we should expect to get nitric oxide, and yet the methods give quantitative yields of ammonia. The reason why nitric oxide was not evolved must be merely a question of reaction velocity; that is, the velocity of the direct reduction (dependent only on the current density) must be so great, compared to the velocity of decomposition of the intermediate compound giving nitric oxide, that this latter reaction is unappreciable. This was tested by repeating Easton's experiments using a current density approximately one hundredth of the lowest value he recommends. Under these conditions no ammonia was formed, even after two weeks of continuous electrolysis."

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[1] J. Phys. Chem., *30,* 1234 (1926).
[2] Z. anal. Chem., *19,* 11 (1880).
[3] Ber., *23,* 2798 (1890).
[4] Z. Elektrochemie, *3,* 546 (1897).
[5] J. Am. Chem. Soc., *25,* 1042 (1903).

Copper does not corrode in a bichromate solution and therefore should not be attacked when made anode in such a solution. Actually, it was corroded practically quantitatively, and it was only when Miss Souders[1] made the current density very small that the chemical and electrolytic reactions became identical. The film making copper passive breaks down under a very small drop in voltage while the corresponding film on iron is quite stable.

Ignoring the significance of the current density got Dennis and Lemon[2] into trouble. They electrolyzed solutions of lanthanum and other rare earth nitrates—in one case nearly pure praseodymium nitrate—at constant voltage and actually obtained a fractional separation. They apparently think that the separation has something to do with the decomposition voltage, because they speak of keeping the voltage "sufficiently constant to permit of fractional decomposition of the different nitrates in solution." Since the voltage drop across the terminals was nine volts, this explanation is quite impossible, because the current density was too high. It is impossible to give the real explanation without repeating the work; but the most probable guess is that they were reducing the nitrates to ammonia. If this is true, the method is slow and crude in comparison with a suitable method of running in ammonia gas.

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[1] J. Phys. Chem., *28,* 831 (1924).

[2] J. Am. Chem. Soc., *37,* 131 (1915).

One case of corrosion puzzled Bengough and Stuart[1]—and others before them—very much. "A very formidable difficulty in the way of the electrochemical theory of corrosion is found in the effect of ions of the corroded metal on the rate of corrosion in a liquid containing them. The presence of these ions, in the case of such metals as magnesium, zinc, aluminum, iron, nickel, and tin, should decrease the rate of corrosion according to the theory, by increasing the osmotic pressure which opposes the solution tension. It has, however, long been known that magnesium chloride is an extremely powerful corrosive agent for magnesium,[2] far more so than distilled water or the chlorides of the alkaline earths or heavy metals, and hydrogen is freely evolved from the metal and does not protect it at all."

Since magnesium cannot be precipitated from an aqueous solution, a magnesium electrode in an aqueous solution of a magnesium salt is not a reversible equilibrium and one cannot apply the Nernst theory to it. This is the same sort of mistake that Carhart[3] made over twenty-five years ago when he wished to consider nickel in a nickel sulphate solution as a reversible electrode. This is, however, a minor matter and the really important thing is why magnesium corrodes more rapidly in a magnesium chloride solution than in distilled water, which everybody admits that it does. It is again a case of the misleading experiment. The different investigators reported accurately on the analytical results and on nothing else. It was, therefore, their sad fate to overlook the really significant phenomena.

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[1] "Sixth Report to the Corrosion Research Committee of the Institute of Metals," J. Inst. Metals, *28,* 61 (1922).

[2] Tommasi: Bull., (3) *21,* 885 (1899); Kahlenberg: J. Am. Chem. Soc., *25,* 380; Roberts and Brown: 841 (1903); Getman: *38,* 2596 (1916); *39,* 596 (1927).

[3] Trans. Am. Electrochem. Soc., *1,* 105; *2,* 122 (1902).

The explanation was suggested by Bryant[1] the same year that Tommasi published his experiments. Bryant studied the action of alcohol, water, and sodium sulphate upon magnesium and found that the rate of reaction increased from alcohol to sodium sulphate. "Magnesium has itself, I submit, the power to decompose water at all temperatures above 0° C., and the action is stopped by an insoluble coating of oxide forming on the metal. This would explain the change of colour seen after the action has ceased, the renewal of action when the surface is cleaned, and the abundant evolution of gas from a solution of sodium sulphate, since magnesium oxide is more soluble in that than in pure water." If Bryant had only known the magic word, peptization, he could have explained everything.

Miss Souders[2] immersed "two rods of magnesium, each 10 cm long and 1 cm in diameter, in pure water and in one per cent magnesium chloride solution respectively. The rod in the magnesium chloride solution became covered with white flakes[3] which were easily removed; the solution was milky, and a considerable precipitate settled to the bottom. The rod which had been in the water acquired a dull, lusterless surface which was moderately smooth; the liquid was clear. The rods were then transposed, the one that had been in the water being placed in the magnesium chloride solution, and the one that had been in the magnesium solution being placed in the water. The rod which was now in the magnesium chloride solution began to flake and become rougher, the film originally produced by the water being obliterated entirely. The rod, which had been transferred to the water became more like the one which had started in the water, the surface seeming smoother and the coating more adherent. It never got really back, because the original pitting had been too severe. There is no question, however, but that the two films are quite characteristic and that the film in water retards corrosion. For this discussion it is immaterial whether one says that magnesium peptizes zinc oxide or not. That is the simplest explanation, because we know that zinc chloride peptizes zinc oxide, ferric chloride peptizes ferric oxide, and chromic chloride peptizes zinc oxide. The essential thing is that there is nothing mysterious about the corrosion of magnesium in a magnesium chloride solution. Any sceptic can repeat the experiments and satisfy himself." This would have been cleared up much sooner if the previous experimenters, with the exception of Bryant, had realized the dangerous possibilities for error in the misleading experiment.

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[1] Chem. News, *80,* 211 (1899).

[2] J. Phys. Chem., *28,* 834 (1924).

[3] Cf. Evans: J. Inst. Metals, *28,* 117 (1922).

Twenty-odd years ago Falk[1] devised a very ingenious method for determining the ignition temperatures for gaseous mixtures. "The ignition temperature was defined as the temperature at which a gaseous mass would ignite if left entirely to itself for some time, the catalytic action of the walls of the vessel in which the gas was enclosed being eliminated. The new method for determining these temperatures consisted of compressing instantaneously the mixtures enclosed in a suitable vessel. This compression raises the temperature of the gas and, if carried out with sufficient rapidity is adiabatic, no heat being lost by radiation or conduction. The rise in temperature of the mixture may then be calculated from the initial and final volumes of the gas, its initial temperature, and the ratio of the specific heats

[1] J. Am. Chem. Soc., *28,* 1517 (1906); *29,* 1536 (1907).

of the gas at constant pressure and constant volume. In compressing the mixtures, as soon as the temperature of ignition is reached there is a sudden reaction (explosion), enough force being developed by the reaction to make the determination of the final volume of the mixture a comparatively simple matter."

For hydrogen and oxygen Falk obtained a smooth curve, with ignition temperatures of about 890° for 80% H_2 and 20% O_2, of less than 800° for 56% H_2 and 50% O_2, and of about 850° for 20° H_2 and 80% O_2. The ignition temperature for the mixture, H_2+O_2, lies lower than that of any of the other mixtures studied. "The most evident explanation for this is that, in the combination of hydrogen and oxygen, the first product formed is hydrogen peroxide which may or may not then be decomposed, the conditions determining how complete the second reaction is."

To anybody who has not worked with explosive gas mixtures, Falk's experiments seem conclusive. They did not seem so to H. B. Dixon.[1] He quotes Falk's statement that the fact that the maximum affinity is shown by the mixture, H_2+O_2, proves that H_2O_2 must be the first product of the reaction between hydrogen and oxygen.

"Falk gives as his reasons for the assumption that the gas detonates instantaneously throughout its whole mass, once the ignition temperature is reached, the fact that the explosions are of great violence, and that the compression was very nearly the same in the case of electrolytic gas whether the weight fell from a lower or a higher altitude. What he found to be nearly true of the rapidly-firing mixtures, $2H_2+O_2$ and H_2+O_2, he has assumed to be also true of the less rapidly-firing mixtures with excess of oxygen, and of mixtures of hydrogen and air.

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[1] J. Chem. Soc., *97,* 661 (1910); *105,* 1091, 2027, 2036 (1914).

"When I read Falk's first paper, it occurred to me that the descending piston must send in front of it sound-waves, which would be reflected from the bottom of the cylinder back to the piston again, and so on, producing by their collisions zones of higher temperature, at one of which the flame would eventually start. Falk states in his second paper that Professor Jouguet has made the same criticism. By photographing the explosion produced by the adiabatic compression on a rapidly moving film, I have shown that the flame *does* start from a point, and in the more slowly burning mixtures the spread of the flame throughout the gas takes an appreciable time. Moreover, the point of ignition can be made to vary by altering the velocity of the piston. With electrolytic gas the photographs show that the explosion wave is very quickly set up, but the flame always starts *from one point*. The mass of the gas *is not fired instantaneously*, although the time required in this case for the flame to reach the piston is negligible. With such a mixture as H_2+2O_2, the time required to set up the explosion-wave is *not* negligible.

"If we accept the Nernst definition of the ignition point of a mixture of gases as that temperature at which a mass of the gas *rapidly* self-heats itself by combination until it bursts into flame, and if this flame starts *at some point* in the mixture and spreads from that point, there will be two periods during which the piston may continue to move after the "ignition point" has been reached: (i) the period from the beginning of rapid self-heating until the flame appears; and (ii) the period required for the flame to spread through the gas and stop the descent of the moving piston.

"Just as the time required for the flame to set up the explosion-wave differs in different mixtures, so does the time required for the self-heating from Nernst's 'ignition point' to the actual appearance of the flame.

"When a mixture of air and hydrogen (5 vols. to 2) was compressed in the cylinder, and the descent of the piston was arrested by means of a steel collar, I found the ignition temperature a few degrees only above that of electrolytic gas. This was in agreement with our previous determinations at atmospheric pressure. But when the piston was allowed to descend until it was stopped by the explosion of gases, as in Falk's experiments, I could obtain ignition points, not only as high as Falk's (649°), but, by increasing the velocity of the piston, I got temperatures of 700°, 800°, or 900° at pleasure. A similar variation was found for the mixture H_2+4O_2.

"It was evident that the error due to the movement of the piston during the 'pre-flame' period of self-heating might be far greater than the error due to the movement of the piston *after* the flame had started.

"In determining the temperature of ignition of mixtures of hydrogen and oxygen, it was necessary, therefore, to stop the descent of the piston artificially the moment the gases were brought to the self-heating temperature. This was effected by having on the end of the piston a steel head, which was caught by a steel collar at a certain point in its descent. By varying the thickness of the collar, the piston could be stopped at any point. By a series of trials a compression was found which fired the mixture, and a slightly less compression, which did not. The mean between the two was taken as the ignition point. On repeating each experiment several times, nearly concordant ignition points were obtained."

"My ignition point for electrolytic gas [$2H_2+O_2$] agrees closely with that found by Falk, an agreement which shows that this mixture must have a very short period of flameless combustion, and the speed of the flame must be very rapid. I find the mixture in equal volumes has a lower ignition point than electrolytic gas, but it is not the mixture of 'maximum affinity.' The further addition of oxygen regularly lowers the ignition point. I would submit, therefore, that the ignition points of hydrogen and oxygen mixtures do not prove that hydrogen peroxide is the first product of the reaction."

Spoehr[1] seems to have been the victim of the misleading experiment in his work on sodium ferropyrophosphate. "The pyrophosphates of iron are soluble in a solution of sodium pyrophosphate. There are thus formed salts in which the iron is present as a complex ion. These salts have been investigated by Pascal[2] who gave to the sodium ferropyrophosphate the formula $Na_8Fe_2(P_2O_7)_3$. This substance is a strong reducing agent, liberating the free metal from salts of gold, silver, mercury, and copper. When ferrous sulphate is added to sodium pyrophosphate, there is first formed a precipitate of ferrous pyrophosphate which, on the addition of more sodium pyrophosphate, dissolves to form a clear, light green solution. Such a solution gives none of the ordinary tests for iron [ferrous] ion, nor does it give a precipitate with alkalies."

Of course Spoehr does not mean that there is a zero concentration of ferrous ion because it would not then be a powerful reducing agent and because iron would dissolve readily in the solution with evolution of hydrogen. As a matter of fact Spoehr calculates in the third paper that the ratio of ferrous to ferric ion may be about 2.4:1. What Spoehr does believe, however, is that the concentration of ferrous ion is below that which can be detected by the ordinary tests for ferrous ion. While this may be true, it does not follow from Spoehr's experiments. Since the usual test for ferrous ion is the addition of potassium ferricyanide, this is undoubtedly one of the tests which Spoehr applied, though he does not say so specifically. As a matter of fact potassium ferricyanide does not give any blue color with a sodium ferropyrophosphate solution; but this does not prove anything in regard to the concentration of ferrous ions. Since the solution has a strong reducing power, it would reduce potassium ferricyanide to potassium ferrocyanide, which would give no test with ferrous ion. We took a dilute ferrous sulphate solution and added potassium ferricyanide to it, getting a deep-blue precipitate. On adding this to the sodium ferropyrophosphate solution, the color bleached at once. Spoehr was using an ordinary test under extraordinary conditions where it means nothing. The test with alkali is positive and shows that the concentration of ferrous ions is below that for a saturated solution of hydrous ferrous oxide.

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[1] J. Am. Chem. Soc., *46,* 1496 (1924); *48,* 107, 236 (1926).

[2] Ann. Chim. Phys., (8) *116,* 386 (1909).

If we add a potassium fluoride solution to a ferrous sulphate solution, there is no precipitation and no visible change. It has been shown by Peters[1] that potassium fluoride takes out ferric ions, forming a ferric fluoride; but has little or no effect on the ferrous ions. If we add a sufficient amount of potassium fluoride, we can then add a potassium ferricyanide solution without getting a blue color.[2] If the local concentration of ferricyanide is too high, a blue color is formed, which disappears on shaking the test-tube. Addition of alkali to solution of ferrous sulphate and potassium fluoride causes a precipitation of hydrous ferrous oxide.

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[1] Z. physik. Chem., *26,* 219 (1898).

[2] This has since been confirmed independently by Szebellèdy: Z. anal. Chem., *75,* 165 (1928).

Sometimes the gods are good to the research man, though not often. The world knew that Sabatier had obtained cyclohexane by the hydrogenation of benzene; but the world did not know until Sabatier[1] told us in 1926 how easily he might have failed.

"This special power of reduced nickel [of hydrogenating acetylene and ethylene] seemed to us so perfect that we thought of generalizing it, and we attacked a case of hydrogenation, which up to that time had not been realized by any method, that of benzene. Berthelot had attempted to effect it with his universal agent of hydrogenation, a concentrated solution of hydriodic acid at 250°. But instead of cyclohexane, which boils at 81° he had prepared only its isomer, methylcyclopentane, which boils at 69°. Under the influence of the hydriodic acid the hexagonal nucleus was transformed into a substituted pentagonal nucleus.

"It was with real emotion that we proceeded to this decisive test. The benzene vapors, carried along by a current of hydrogen in excess, were directed over a bundle of reduced nickel maintained at 200°; the gases issuing from the tube were sent to a U-tube surrounded by ice, within which the vapors of cyclohexane should be expected to condense in a liquid state. The operation seemed to proceed well from the beginning, the velocity of hydrogen issuing from the condenser tube seeming very little less than that at the entrance. But after boiling for a rather short time, the gas ceased to pass, the U-tube was stopped up by the condensation of a solid product, which it seemed could only be benzene, solidifiable at 4°—that is to say, within the ice—whereas we had seen in the literature that cyclohexane, extracted from Caucasian petroleum by Markounikov by means of 110° fractional distillations, was condensed to a solid which melted at −11°. But our despair was of short duration and was changed to a very lively joy, when on opening the U-tube we perceived, instead of the odor of the original benzene, the special intermediate odor between that of chloroform and that of the rose, which belongs to cyclohexane. The condensed product was cyclohexane, entirely pure, melting, not at −11°, but above 6°, absolutely unattacked by the sulfonitric mixture. The transformation of benzene had been complete. . . .

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[1] Ind. Eng. Chem., *18,* 1007 (1926).

"Either with M. Senderens or with two others of my pupils, M. Mailhe and Murat, I have been able to accomplish with success more than two hundred different hydrogenations. These direct fixations of hydrogen in the presence of reduced nickel in the neighborhood of 180° are usually very easy and frequently give very high efficiencies without any difficulty. But this is accomplished only by using carefully prepared nickel and pure hydrogen, and avoiding the presence of any substance capable of forming with the nickel a combination irreducible by hydrogen, thus diminishing the active surface of the catalyst. The rôle of such a substance is analogous to that of microöganisms in fermentations. A poison introduced into the liquid kills the ferments and suppresses their useful action. In the same way the metallic ferment, which is the nickel catalyst, is killed by traces of chlorine, bromine, sulfur; and thus neglect of sufficient precautions explains the failure that so many chemists experience in certain hydrogenations such as those of benzene and phenol.

"I should acknowledge that in this respect M. Senderens and I have had very good fortune. Naturally we were ignorant at the beginning of our labor, of the extreme susceptibilities of the metal catalyst and from the first, instead of ourselves preparing the oxide destined for the reduction, we made use of an oxide bought on the market, which fortunately was found good. By good fortune also, our benzene and our phenol, bought it is true as pure, were so in reality. But if there had been in the benzene a small portion of thiophene or chlorobenzene, we would not have obtained any hydrogenation and, discouraged by this accidental failure, we would perhaps have attributed it to the direct impotence of nickel in this particularly difficult case of hydrogenation."

It was his previous experience which enabled Langmuir[1] to recognize and rise superior to the misleading experiment. "By this time I was familiar with all the harmful effects which gas can produce in contact with filaments and knew under what conditions these bad effects could be avoided. In particular, I realized the importance of avoiding even almost infinitesimal traces of water vapor. Thus, when I found a marked effect of mercury vapor and nitrogen in reducing the rate of evaporation, it occurred to me that it might be possible to operate a tungsten filament in gas at atmospheric pressure and obtain a long useful life. Of course, it would be necessary to raise the temperature far above that at which the filament could be operated in vacuum in order to compensate for the serious loss in efficiency due to convection by the improved efficiency resulting from the rise in filament temperature. Whether or not the increased rate of evaporation, due to this increase in temperature, would more than offset the decrease in the rate due to the gas was a matter that could only be tested by experiment. . . .

"Calculations showed that it was hopeless to get practical lamps with filaments in nitrogen, if these filaments were of very small diameter. . . . With filaments of much larger diameter, however, the effect of the gas in decreasing the efficiency was not nearly so marked. We therefore constructed lamps having filaments of large diameter in the form of a single loop and filled these lamps with nitrogen at atmospheric pressure. We ran these lamps with a filament temperature so high that, in spite of the gas, the efficiency corresponded to about 0.8 watt per candle instead of the usual one watt per candle at which we tested our vacuum lamps. We were disappointed to find that these lamps blackened much more rapidly than vacuum lamps of similar efficiency so that the total useful life of the lamp was short.

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[1] Ind. Eng. Chem., *20,* 334 (1928).

"This result, which is what most lamp engineers would have expected, seemed to indicate that the rise in temperature necessary to offset the heat losses by the gas increased the evaporation by more than the amount of the reduction in evaporation due to the gas. If I had not previously become so familiar with the behavior of various gases, this discouraging result might easily have stopped further experimenting in this direction. However, I noticed that the bulb had blackened during the short life of the lamp, whereas from my knowledge of the interaction of tungsten and nitrogen I had expected a deposit of a clear brown color. I felt that the black deposit could mean only one thing, water vapor, notwithstanding the fact that, to avoid this water vapor, we had taken precautions which were greater, I believe, than had ever been used before for the preparation of moisture-free gases and glass surfaces. We were thus led to take still greater precautions and use still larger bulbs, so that the glass should not become overheated by the convection currents in the gas that rose from the filament. We were then soon able to make lamps having a life of over 1,000 hours with an efficiency about 30 to 40 per cent better than could have been obtained with filaments in vacuum.

"As I look back upon these experiments, I think that we were very fortunate at that time in not having had at our disposal a supply of argon gas. From theoretical reasons I had concluded that argon should be better than nitrogen, and if I had had argon I should have tried it first. If these lamps had blackened because of traces of water vapor, I would naturally have attributed this to the increase in evaporation caused by the high temperature, and would have had no reason for suspecting that water vapor was the cause of the trouble, for, of course, in argon a brown deposit would not be expected in any case."

The attempted transformations of the elements[1] have given rise to a good many misleading experiments. "A brief statement will be given of a number of experiments made by Cameron and Ramsay[2] in order to test whether ordinary matter can be transformed by the radiations. The question is a difficult one on account of the smallness of the effects to be expected. Several positive results have been published; but these have not stood the test of more rigorous and careful examination. Ramsay and Cameron first investigated the action of the radium emanation on water. For this purpose, a large quantity of the emanation was added to a small bulb partly filled with water. In this case part of the emanation was absorbed in the water and part remained in the gases formed above it. They concluded that not only did the emanation produce helium but also neon and a small trace of argon. The experiment was repeated by Rutherford and Royds[3] who found evidence only of helium. The neon and argon observed in the experiment of Cameron and Ramsay were undoubtedly derived from the small quantity of air which admittedly leaked into the apparatus. Neon is so sensitive spectroscopically that by suitable methods its presence in 1/10 c.c. of air can be easily observed. Cameron and Ramsay also observed that the radium emanation added to a solution of salts of copper gave rise to small quantities of sodium, potassium, and lithium, and concluded that lithium was produced by the transformation of copper. The experiments were carefully repeated by Mme. Curie and Mlle. Gleditsch,[1] who were unable to confirm the results. The experiments are difficult, since traces of lithium are ordinarily present in many substances. They found, for example, that, when pure water had rested for some time in a glass vessel and then evaporated, a small residue was obtained consisting chiefly of sodium, but also containing a small quantity of lithium. Similar results were observed with quartz. To avoid these difficulties, experiments were made with platinum vessels, and in this case the presence of lithium could not be observed.

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[1] Rutherford: "Radioactive Substances and their Radiations," 321 (1913).

[2] J. Chem. Soc., *91,* 1593 (1907) *93,* 966, 992 (1908).

[3] Phil. Mag., (6) *16,* 812 (1908).

"The question was attacked in another way by Mlle. Gleditsch[2] and by McCoy[3] by determining the amount of lithium in radioactive minerals containing copper. If copper was transformed into lithium by the radiation, it would be expected that there would be some relation between the proportions of copper, radium, and lithium in the various minerals of about the same age, but no evidence was obtained of any such relation. Ramsay and Usher[4] stated, later, that carbon dioxide was produced by the radium emanation in solutions of thorium and zirconium. It is well known that the emanation gives rise to the carbon dioxide when any organic matter is present, and it is very difficult to prove that carbon dioxide is derived from the atomic transformation of thorium and zirconium."

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[1] Compt. rend., *147,* 345 (1908).

[2] Ibid., *145,* 1148 (1907); *146,* 331 (1908).

[3] Nature, *77,* 79 (1907).

[4] Chem. News, *100,* 209 (1909); see also Ramsay: Compt. rend., *153,* 373 (1911).

Paneth[1] calls attention to other cases in his introductory address as the George Fisher Baker Non-resident Lecturer at Cornell University. "You may have heard that just before the war, Collie and Patterson announced, from the laboratory of Ramsay, that helium and neon were formed when an electric discharge was sent through hydrogen that was in a closed vessel. But shortly afterwards, Strutt, the present Lord Rayleigh, showed that this experiment could not be repeated if air, which always contains both helium and neon, was perfectly excluded. Nevertheless, the assertion that they are thus formed was repeated a few months ago in the Proceedings of the Royal Society of London, but this experiment fails to convince one that Strutt's criticism is incorrect. In experiments, as yet unpublished, which were carried out by Dr. Peters and myself at the University of Berlin, the entire apparatus was submerged in water which served so to cool the glass walls as to render them impermeable to atmospheric helium. After hours of continuous electric discharge, we found no trace of helium.

"Many of you are doubtless familiar with the fact that last year a German chemist Miethe, and a Japanese physicist, Nagaoka, independently asserted that gold may be formed from mercury by electric discharge. It is a special irony of fate that in this case alchemy reappeared in the old-known vestment of the artificial production of gold. Gold is particularly apt to cause this error, because it may be present in various materials but so finely distributed as to escape detection by ordinary methods of analysis. But in various ways the gold may be concentrated to such an extent that it can then be detected without difficulty. Concentration was, therefore, in all the ages very often misunderstood as production. In the experiments of Miethe and Nagaoka quite the same happened. Today extremely small quantities of gold can be recognized and the scarcely visible beads of gold which Miethe could identify under the microscope would have entirely escaped the observation of the alchemists of the seventeenth century. But if in the final product we are satisfied with such small quantities of gold, this necessitates the employment of much more delicate methods of ascertaining that the original material is perfectly free from gold. Hence the difficulties and the danger of error have been about the same in all centuries. That this modern 'transmutation,' like that of old, amounted only to a concentration of gold and not to its genesis, has been proved by Tiede, Riesenfeld, Haber, and their co-workers in Germany and by Sheldon, Estey and Harkins in this country."

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[1] "Radio-Elements as Indicators," 16 (1928).

In spite of his knowledge of what had happened to many of his predecessors in the same field Paneth[1] believed that he had converted hydrogen to some extent into helium by means of finely-divided palladium, only to find that, like the others, he had been the victim of the misleading experiment.

Although people have taken great pains to determine physico-chemical constants, the tests for purity have been so inadequate in the past that Cohen[2] stated, not long ago, that "most, if not all, hitherto determined physico-chemical constants of solid substances are in need of a revision, since it is very probable that the present values refer not to the physically and chemically pure modifications, but to physically impure and metastable mixtures which contain the respective forms in unknown proportions. This state of affairs opens up an immense field of research to scientists and investigators, and one which may yet have many a surprise in store for us."

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[1] Naturwissenschaften, *14,* 956; Ber., *59* B, 2039 (1926); *60* B, 808 (1927).

[2] "Physico-chemical Metamorphosis and Some Problems in Piezochemistry," 94 (1926).

We are always told that pectin will not form a jelly with sugar and water, and that acid is necessary. Consequently, people have started with that as a fundamental in working out the theory of jelly-making. The statement is true, but entirely misleading. It is true if one works with about one-half per cent of pectin. It is not true if one works with one per cent of pectin.

In 1877 Downes and Blunt[1] showed that ultraviolet light destroys many bacteria.[2] "They thought that they had established the necessity for the presence of free oxygen. In this they were undoubtedly in error. This error persisted for a long time, being repeated and emphasized by Roux in 1887, until it was corrected by Marshall Ward in 1892. It is instructive to consider the causes for this mistake. The early experimenters used the whole radiation of the sun, or of the arc lamp. When attempting to test whether the exclusion of oxygen would prevent the sterilizing action, they adopted one of two methods. Usually, they enclosed the test culture in an exhaustive glass tube and exposed this to the rays. Now we know that glass absorbs all but the longest of the ultraviolet rays: and that although the investigator had eliminated oxygen, they had simultaneously shut out the active rays. Occasionally they caused the rays to fall normally to the surface of a culture on a solid medium. Observing that in this case only the upper layers were sterilized, they concluded that the deeper strata were protected by the absence of free oxygen. We know that the active

[1] Proc. Roy. Soc., *26,* 488 (1877).

[2] Ellis and Wells: "The Chemical Action of Ultraviolet Rays," 237 (1925).

rays have scarcely any power of penetrating organic films, and that this is the true explanation of the limitation of the sterilizing effect to the surface layers of the culture. . . .

"The germicidal action[1] is concentrated in the ultraviolet, somewhere between $\mu\mu 340$ and $\mu\mu 185$. Ward discovered that the action was just as vigorous in the absence of free oxygen as in the air. This has, since, received ample confirmation. Oxygen, in the free state, is, therefore, not necessary for the sterilization action of ultraviolet rays."

Wheeler and Wells[2] considered that they had proved definitely the existence of solid KI_3, stable at 25°. The substance had a different crystalline form from potassium iodide, columns instead of cubes, and analyzed for potassium tri-iodide. If one puts cubes together, one on top of another, one gets a column. The crystals were not of a different form but only seemed to be. When the crystals were dried between filter paper, the latter adsorbed potassium iodide in preference to iodine and did not remove the mother-liquor as a whole. The crystals were really potassium iodide stained by iodine; but a faulty crystallographical identification and a faulty analysis seemed to prove conclusively that they were potassium tri-iodide. The probabilities are all against two errors giving such results; but the thing did happen.

Posnjak[3] points out that "among the large number of substances which have at one time or another been X-rayed, the record for potassium is, in a way, a singular one. According to the record no regular diffraction of X-rays, but only a so-called general scattering, is obtained at room temperature by the usual powder method. The first to investigate the crystal structure of the alkali metals was A. W. Hull.[1] He reports encountering considerable difficulties in obtaining X-ray diffractions from sodium and lithium and the failure to obtain any from potassium. The observation that no diffraction was obtained from samples made from freshly melted sodium and lithium and that even samples made from old stock produced a large amount of continuous scattering led him to believe that sodium and lithium were only partly crystalline and that potassium must be completely amorphous. Later McKeehan[2] re-investigated potassium. He found that, if the potassium is cooled to −150°, weak reflections are obtained, as from large crystals; however he says that this crystalline structure does not persist when the temperature is allowed to rise to about 20°.

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[1] Ward: Proc. Roy. Soc., *52*, 393 (1892).

[2] Am. J. Sci., (4) *43*, 476 (1892); Johnson: J. Chem. Soc., *31*, 249 (1877).

[3] J. Phys. Chem., *32*, 354 (1928).

"The conclusion derived from X-ray experiments, that potassium must be amorphous at room temperature, is very surprising, as the fractured surface of potassium shows brilliant facets; and well-defined crystals have been grown even from the molten metal;[3] and further, of course, because potassium has a definite and sharp melting and freezing point. It is not quite conceivable that an amorphous substance could possess such properties, and R. W. G. Wyckoff,[4] in discussing this case in his book on 'The Structure of Crystals,' tries to explain these conflicting facts by the assumption, not that potassium is an amorphous substance, but that the amplitudes of the thermal vibrations of potassium atoms at room temperature are so great as to 'obliterate the crystalline diffraction maxima.' We do not need, however, to go into the details of this discussion, as it will be shown that the assumption is unnecessary. The reason for mentioning this is chiefly to call attention to the danger of drawing definite conclusions and of putting too much reliance on a few negative results of X-ray experimentation.

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[1] Phys. Rev., (2) *10,* 661 (1917).

[2] Proc. Nat. Acad. Sci., *8,* 254 (1922).

[3] Long: J. Chem. Soc., *13,* 122 (1860); Schroedler: Ann., *20,* 2 (1836); Pleischl: J. prakt. Chem., *31,* 45 (1844).

[4] "The Structure of Crystals," 372–373 (1924).

"The present investigation grew out of experiments on non-crystalline substances of which potassium was supposed to be one. Contrary to earlier statements, every sample of potassium that was X-rayed in this investigation showed normal diffraction effects. In view of this, and for comparison results, a few preparations of sodium and one of lithium were made and X-rayed. These substances also in each case gave definite diffraction effects, which were in good agreement with previous determinations of their crystal structure. . . . The reason for obtaining only a general scattering and no definite diffraction in former experiments is thought to be the formation of films on the sample of potassium by moisture, by oxidation, or by interaction of the potassium with the glass."

It seemed perfectly natural that the converting of milk into a powder would destroy vitamin C and this belief was considered to be proved when a sample of milk powder was found on analysis to be low in vitamin C. This is a case of the misleading experiment. The observation was right; but the obvious conclusion to be drawn from it was wrong. The catch in the case is that the amount of vitamin C in the milk varies with the time of year because of the difference in feed. Milk powder made from milk low in vitamin C will itself be low in that vitamin. This has been brought out clearly in an investigation carried out at Cornell by Professor Cavanaugh.[1]

"On account of the conflicting statements in the literature regarding vitamin C content of dried milk products, and especially in view of definite statements regarding the effect of the spray process on vitamin C, a special investigation of this problem has been conducted.

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[1] Cavanaugh, Dutcher and Hall: J. Children's Diseases, *25,* 498 (1923); Ind. Eng. Chem., *16,* 1070 (1924).

"This investigation was started in February, 1922. The method consisted in feeding young guinea-pigs a standard basal diet free from vitamin C. To this was added raw liquid milk or spray-process powdered milk in varying amounts, these milk products being the only source of vitamin C in the diet.

"The milk, both liquid and powdered, was shipped by express daily from the Merrell-Soule Powdered Milk Factory at Perry, N. Y. The milk, both raw and powdered, which was used in this experiment was taken from the mixed milk of 175 dairies representing 1,276 cows. The raw milk for feeding was drawn each day from a large vat. In order to prevent souring of the raw milk, it was cooled and shipped in sterilizing bottles. The milk in the vat was pasteurized under conditions known to be favorable to the preservation of vitamin C and then reduced to powder. The powdered milk for feeding was drawn each day from this powder so that the raw milk and powdered milk came from the same lot of fresh milk. Unless this precaution is taken, a fair comparison between raw and powdered milk cannot be made, as the vitamin C content of different lots of fresh liquid milk has been found to vary considerably. The experiments on winter milk were kept separate from those on summer milk on account of a similar variation.

"This difference between summer and winter milk was confirmed by the present experiment. The fact indicates that no reliable conclusions regarding the effect of drying processes on the vitamin C content of milk can be drawn unless comparison is made between dried milk and fresh raw milk taken from the *same* lot. Failure to take this precaution may probably account for some of the contradictory reports previously published."

"A study of the groups at the end of sixty-one days shows that the control group developed scurvy more quickly than the groups fed raw or powdered milk. Each group fed powdered milk did at least as well as the group receiving an equivalent amount of raw milk. So far as we can judge from data obtained from these experiments with guinea-pigs, there appears to be no destruction of the antiscorbutic properties of milk when it is thus powdered. This confirms the conclusions drawn from our first experiment; i.e., that milk powdered by this form of spray process possesses as great antiscorbutic properties as the raw milk from which it was made."

Since winter milk is notably deficient in vitamin C, the use in the winter of milk powder made from summer milk would give the child more vitamin C than would the use of raw winter milk—always supposing that there is no serious loss of vitamin C in milk powder which has been kept nearly a year.

I am indebted to Dr. J. E. Mills of Edgewood Arsenal for a striking case of the misleading experiment. The Chemical Warfare Service was trying the effect of toxic gases on the boll weevil. Naturally they began with a relatively high concentration of gas so as to be sure of killing the weevil. Thirty minutes exposure to a fairly concentrated hydrocyanic acid gas produced very few casualties among the boll weevils and the obvious conclusion was that the boll weevil is extraordinarily insensitive to hydrocyanic acid gas. The more Dr. Mills thought this over, the less probable it seemed. Dr. Mills and his staff decided that the boll weevil recognized in some way the unpleasant possibilities of concentrated hydrocyanic acid gas, and that he shut up like a clam, refusing to breathe while the experiment lasted. Of course if the weevil did not breathe the gas, it would not hurt him. It was decided to try a relatively low concentration of hydrocyanic acid gas in the hope that the boll weevil would not recognize its danger until it was too late. The experiment was successful and the boll weevils were killed by the low concentration of hydrocyanic acid gas, though unharmed by the high concentration. The boll weevil is quite sensitive to hydrocyanic acid gas provided the gas gets to it.

Dr. M. J. Brown of the Roessler and Hasslacher Chemical Company has cited the reverse case with some scale insects on citrus trees. A relatively high concentration of hydrocyanic acid gas killed the eggs of the scale; but a low concentration stimulated the eggs so much that a larger percentage hatched than in the control groups. People had not considered this possibility in the case of scale insects; but it is not very surprising when we stop to think about it. Many of us have taken small doses of strychnine at one time or other to stimulate digestion. Over seventy-five years ago certain English brewers were accused of taking an unfair advantage of their competitors by adding strychnine to their pale ales. The fact that the charge was not true is of no importance now, though it was then. While small doses of strychnine may be and often are beneficial, larger doses are invariably painful and fatal.

In the non-resident lectures at Cornell University under the George Fisher Baker Foundation, Professor Barger, of the University of Edinburgh, stated that Zuetzer called an insulin preparation toxic because he gave an over-dose to a dog, and that was toxic.

When I lectured on "The Misleading Experiment" at the

University of Indiana, Professor Weatherwax[1] called my attention to experiments running over many years which are certainly cases of misleading biological experiments.

"An unusually fascinating problem met in a botanical study of the Indian corn plant is that pertaining to its origin and relationships. Since this cereal has been known to the white man, no plant that seemed to be its wild ancestor has ever been seen. Either the centuries of aboriginal domestication have so changed the cereal that we do not now recognize its wild progenitors, or the latter has disappeared or become extremely rare. It is appropriate, then, that the chasm be bridged in theory, and the theories and hypotheses that we have in this connection are many. The merits of some of these have been discussed elsewhere.[2] It is my intention to deal critically here with only one, which has, unfortunately I believe, gained wide popular publicity. This theory is all the more interesting because it was arrived at independently by two widely separated workers, one of whom is adapted to give it publicity and the other to give it the stamp of scientific approval.

"When we look over the list of species that the taxonomist has grouped with Indian corn we soon eliminate from close relationship all but one. This is teosinte (*Euchlaena mexicana* Schrad.), which still grows wild in some parts of Mexico. This plant resembles corn so closely in vegetative characteristics and in staminate inflorescence that most persons pass it by as merely a peculiar kind of corn. In the pistillate inflorescence, however, the difference between it and corn is readily seen. When teosinte first became generally known to the scientific world, it was thought that it might be wild corn. In many respects, however, teosinte is too highly specialized to stand in such relationship, and most investigators have looked in other directions for the answer to the question."

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[1] Proc. Indiana Acad. Sci., *34,* 225 (1925).

[2] Weatherwax: Bull. Torrey Bot. Club, *45,* 309 (1918).

Luther Burbank believed that, after eighteen years of selection, he had changed teosinte to corn. Bento de Toledo believed that he had done the same thing after only five years of work in Brazil. "Neither Burbank nor Toledo has anything definite to say about the exact nature of the material with which he started the series of selections. It was merely 'teosinte,' and beyond this their figures are allowed to speak for themselves. But it must be remembered that teosinte hybridizes freely with corn, and hybrids of all degrees are fertile among themselves and with either parent. Neither is there any mention of precautions to prevent contamination of the stock by wind-blown pollen of corn during the experiment."

A comparison of the drawings with that of what is believed to be pure teosinte seems to Weatherwax to be conclusive. "There seems to be little room for doubt that the breeding stock used by both Burbank and Toledo was already as much corn as teosinte and that both have simply selected out the characteristics of corn possessed by the hybrid. That both did change the nature of the stock used is not to be doubted. But, on the basis of the figures cited, it is the writer's opinion that the probable impurity of the original stock and its possible contamination with corn during the experiment are sufficient to invalidate any conclusions that may be drawn as to the ancestry of Indian corn."

Physical chemists have been calculating hydrogen ion concentrations for years on the assumption that the solution pressure of hydrogen was the same in all aqueous solutions. We now know that it is possible to make up solutions in which this method of calculation will give a five hundred per cent error owing to the displacement of the water equilibrium.[1] This is not so serious in itself because nobody is especially interested in solutions containing a little hydrochloric acid and a great deal of sodium chloride. What hurts is that we do not know the magnitude of the possible error in any case.

Luther and Forbes[2] studied the effect of light on the reaction between quinine and chromatic acid, finding that the photo-chemical activation of chromate must be negligible compared with that of quinine. In a later paper Forbes and Leighton[3] have failed to find any effect of light from a mercury lamp on the electrolysis of a chromatic acid solution. Plotnikow[4] objected to findings which belittled the photoactivity of chromates, and I wish to join in the protest. While Luther and Forbes undoubtedly found what they said they did, that does not make their experiments any less misleading. There can hardly be any question of where Forbes stands, because Forbes and Leighton speak of testing further "the alleged activation of chromates by light." This is quite unintelligible in view of the fact that there are technical processes depending on this very thing.[5]

"When mixed with organic substances, potassium dichromate is reduced on exposure to light; gelatine under such circumstances is rendered insoluble. This reaction is taken advantage of in the 'Carbon' process of photography. The gelatine is mixed with a pigment of any colour and the paper carrying this film is sensitised by floating on a solution of potassium dichromate. On exposure under a negative, the gelatine becomes insoluble in those portions exposed to the light, and retains the pigment, while the portions protected by the darker parts of the negative are almost unacted upon, and may be dissolved in warm water. In this manner photographs of any desired colour, and of great beauty and permanence, may be produced. . . . The insoluble gelatine does not absorb water, but will take oil, which may be mixed with any desired pigment and this forms the basis of the oil and bromoil processes. Gum, mixed with dichromate, is also made insoluble by exposure to light, and this is made use of in photography in the gum dichromate process."

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[1] Bancroft: J. Phys. Chem., *30,* 1194 (1926).

[2] J. Am. Chem. Soc., *31,* 770 (1909); Forbes, Woodhouse and Dean: *45,* 1891 (1923).

[3] J. Phys. Chem., *30,* 1628 (1926).

[4] "Allgemeine Photochemie," 215, 416, 583 (1920).

[5] Thorpe: "Dictionary of Applied Chemistry," *2,* 241 (1921).

Plotnikow has shown that ammonium bichromate oxidizes alcohol in the light. In the Cornell laboratory, Mr. Morton[1] has found that a solution of potassium dichromate in aqueous alcohol is decomposed—as of course it must be—by light which has passed through a filter made of copper sulphate solution. It seems probable that Forbes obtained his misleading results by working with solutions which were so strongly acid that no marked increase in oxidizing power could be detected. In his electrolysis experiments his solutions contained normal and three-normal sulphuric acid—which was certainly riding for a fall. In the experiments with quinine and chromic acid the sulphuric acid was 5.4 normal.

In Fittig's laboratory at Strassburg, Pagenstecher[2] found that addition of bromine to tiglic acid and to angelic acid gave the same dibromvalerianic acid. In Wislicenus' laboratory at Leipzig, Puckert[3] obtained two isomers, which he called tiglic acid dibromide and angelic acid dibromide, the latter being new. This started a long dispute, because Fittig[4] claimed that it was impossible that such an error could have been made in his laboratory. The matter was cleared up by Wislicenus,[1] who showed that the nature of the addition product varied with the illumination.

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[1] J. Phys. Chem., *33,* 1135 (1929).

[2] Ann., *195,* 123 (1879).

[3] Ann., *250,* 240 (1889).

[4] Ann., *259,* 1 (1890); *273,* 127 (1893); *283,* 65, 107 (1894).

"That Pückert obtained this isomer [angelic acid dibromide] of the tiglic acid dibromide, which latter had been prepared pure in several ways by Fittig and his pupils, and that this compound was not discovered years before in Fittig's laboratory, was due to an unnoticed detail. In the upper rooms of the Leipzig laboratory, almost all the hoods were in the outer walls *between* the windows, and were therefore dark. In the Strassburg laboratory, the hoods were *in* the windows, and were therefore filled with the brightest light. Without thinking about it, Pückert had therefore done his addition of bromine to angelic acid in the absence of bright light and obtained a product consisting chiefly of angelic acid dibromide, while Fittig and his pupils obtained chiefly tiglic dibromide because they worked—also without recognizing it consciously—in bright sunlight, the other conditions being practically the same. If the internal arrangements of the Leipzig laboratory had been like those of the Strassburg laboratory, the repetition of Pagenstecher's work in the Leipzig laboratory would have confirmed his results at least temporarily. The true angelic acid dibromide would have been discovered before now in any case. In some still unpublished work done four years earlier but interrupted by the present paper, I had noticed the great effect of varying illumination on the yield of the geometrically isomeric dibromides in the case of the bromine addition products of tolane. It was therefore intended to repeat Pückert's work with this in mind."

A tragic case of the misleading experiment, or of the failure to work out cause and effect, has been cited by Jarvis, Clough and Clark.[1] "Goiter is a disease of world-wide distribution, but most especially of the temperate zone. It is most prevalent in districts far removed from the sea and in high mountain ranges, such as the Himalayas, Alps, Pyrenees, Andes, Cascade and Rocky Mountains. A very graphic report is given by Keith[2] whose account of the incidence of goiter in the Pemberton Valley of British Columbia furnishes a striking illustration of this point. This valley, some 40 to 50 miles long, lies in the mountains of the Coast Range, about 90 miles north of Vancouver, B. C. It is watered directly from the Lillioet river, which rises from a glacier near by. In former years every white baby born in this valley had a goiter, and women developed goiter within a few months after arrival. It was especially severe on livestock. There was a mortality of 90 per cent of all calves born, 85 per cent of the colts, and no pig survived more than one day after birth. Abandonment of the valley was seriously considered at one time. It will thus be seen that this question is not only one of public health but of economic importance."

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[1] Ann., *272,* 7; *274,* 99 (1893).

"As regards chickens, ducks, and, I believe, turkeys as well, all brought into the valley appeared to thrive; but their eggs, though producing embryos, failed to hatch out, the apparent cause being the thickening of the white envelope inside the shell which was so thick and rubbery that apparently the young chick could not break its way through. Occasionally, too, a full-grown hen would suddenly drop dead, or a rooster lose his voice."

"A village of Indians is located at the lower end of the valley at the junction of the Lilloet and Birkenhead Rivers, situated in this same spot as far as the memory of man goes. None of the Indians have goiter, and they have no knowledge of any of their tribe ever having had goiter. . . . If insanitary conditions have a connection with goiter, as is contended by McCarrison, then one would expect a high incidence of goiter among these Indians, who have not the faintest conception of sanitation. That the freedom from goiter of this particular group is not a question of race is shown by Clark and Pierce, who state that Indians living in Minnesota, Michigan and Wisconsin had as high an incidence of goiter as white inhabitants in the same district."

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[1] "University of Washington Publications in Fisheries" (1926). Iodine Content of the Pacific Coast Salmon.

[2] Canadian Med. Ass. J., *14,* 284 (1924).

While one might wish to make the improbable assumption that this tribe of Indians had perhaps, after many generations, become immune to goiter in some mysterious way, that seems to be barred out by the fact that "only rarely have their pigs been found to suffer from myxœdema, though the pig is susceptible to diseases of the thyroid." If the Indian can raise pigs and the white man can raise none, there must be a local reason.

Keith's explanation sounds plausible. "Whilst considering the lack of goiter among these Indians, I would like to draw attention to the fact that they eat a great deal of salmon. The fish come up the Birkenhead to spawn, and many millions of eggs are secured at the Government Hatchery a mile above the village. The Indians are allowed to use the spent salmon, and annually cure thousands for winter use. Their pigs also eat the dead salmon washed ashore on the gravel banks of the stream. It is quite probable that the Indians and their pigs get enough iodine from the salmon to give their thyroids the necessary quantum of this element."

It is almost like a fairy story; but it happens to be true.

The white inhabitants of the valley then took up eating iodized food and lived happily ever afterwards, they and their animals.

"Confronted then with this grave economic problem, Ronayne [an Irishman of good old yeomanry who, with his two brothers have farms about fourteen miles up the valley] applied for advice to the Bureau of Animal Industry at Washington and was referred to Dr. Marine of Cleveland, as the best known authority on the subject. Marine informed him that the cure was very simple, namely the administration of tincture of iodine or potassium iodide to the animals. He suggested five drops a week for horses and cows, three drops for sows, and a few drops for chickens. The iodine acted in a miraculous way. All the goiters disappeared from his animals and no further trouble in rearing stock or fowl has occurred, and in October, 1922, when the writer visited the valley he could not find a goiter in an animal or human being. . . .

"Acting on Dr. Marine's suggestion, Ronayne gave a cow that had previously had four goitrous calves (this cow was born in the valley, and had shown no goiter herself) five drops of tincture of iodine twice a week, beginning the treatment one month before the calf was due. The result was a perfectly healthy calf. After some experience with giving iodine, Ronayne found that very little was necessary in summer and that the most necessary time to give iodine was in winter and early spring. He mixes fifty drops with salt, giving this occasionally to the whole herd during the winter months. This entails very little labour, but he found that a weak member of the herd would sometimes get pushed aside and not get its required quota. Mares in foal get five drops of the tincture a week, beginning two months before foaling time. Pigs require the least, and no iodine is necessary if they are getting skimmed milk from cows receiving iodine. Ronayne gives iodine to the chickens only in spring when eggs are being gathered for hatching and then a few times is sufficient, mixed with mash or in the milk. It is only given as a preventive measure as his hens often get skimmed milk. It costs Ronayne for iodine to keep 100 cattle, 12 horses, 30 pigs, and 200 chickens free from goiter, and their progeny free also, $2.00 worth of tincture annually.

"Any resident of the valley exhibiting a goiter takes a little iodine till it disappears. If the cows are getting iodine and the children and grown-ups are drinking from these cows, it is not necessary for them to take iodine direct."

A practical test extending over more than a quarter of a century seemed to indicate conclusively that the Pemberton Valley was uninhabitable for white people and their animals. This result was entirely misleading, as it has now been proved that the whole trouble was due to a deficiency of iodides. On the other hand we must not let this new set of facts mislead us as to what has actually been proved. It has been proved that administering a suitable amount of iodine is of tremendous value in a goitrous district. A natural corollary of this would seem to be that small doses of iodine might be good for everybody as a precautionary measure. Acting on this assumption many people in New York City are using iodized salt. While this may be desirable, Dr. Keith[1] presents some evidence which raises some doubts.

"A number of observers in Europe and on this continent have drawn attention to the harmful effects that may result under some circumstances from continued use of iodine. They have found that when it is employed for the prevention of goiter without due medicinal supervision, used in quack medicines or taken as iodized salt in irregular amounts

[1] Canadian Med. Ass. J., *16,* 1171 (1926).

for a long period, serious toxic symptoms may be provoked.

"Birker, of Aarau, Switzerland, states that the carrying out of systematic iodine treatment has its dangers. Although he favors the use of iodine as a preventive measure, he thinks that it will take almost a lifetime before we know its action completely. He states that the immediate result in Switzerland has been a great increase in the number of toxic goiters; that at least 2,000 Swiss people have consulted physicians for the symptoms provoked by iodized salt; and that, in his own clinic, where operations for goiter formerly showed 2 to 5 per cent of cases with toxic symptoms, now his records show that 20 to 25 per cent are toxic cases; some clinics in that country report as high as 50 per cent of toxic cases."

I do not know whether detective stories are permissible evidence in a scientific discussion; but I read them and I wish to close with a quotation from one.[1] "The evidential value of any fact is an unknown quantity until the fact has been explained."

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[1] R. A. Freeman: "The Singing Bone," 254 (1923).

III

HOW TO THINK

THERE are two kinds of thinking, the conscious and the unconscious. We begin of course with conscious thinking. We chemists spend a great deal of time practising our students in manipulative skill; but very few of us ever do anything in the way of teaching them to think. Many people do not realize it; but it is not easy to think efficiently. Two very difficult things which I have to do are to teach graduate students to ask why and how to think. I suggest to a graduate student that he try a certain experiment and I tell him why I expect it to go a certain way. After a while he comes back and says that the experiment does not go. I ask him why and he looks grieved. That is not his job. It takes at least a year to get him to asking why. Since all small boys ask why, the result of intensive training is to put a boy further back in some respects than when he started.

In about a year one can cure a good man of not asking why; but it takes a great deal longer than that to teach him to think properly. Most people do research by making a guess, testing it, making another, testing that, and so on. The results obtained are valid only for those conditions and usually are not corrected for the misleading experiment. When one gets through, one knows nothing more than that the particular guesses were not right. It is like four people hunting as they usually do for a golf ball. After five minutes of that, all one knows is that the golf ball has not been found; but one does not even know where the ball is not, because the ball may have been within an inch of where one was looking. If the four people plus caddies line up and walk down a stretch, one at least knows that the ball is not there at all, which is something.

During the war I happened to be connected with a group which was asked to report on a difficulty in regard to the chemical manufacture of ammonium picrate. Instead of being all one color, the salt crystallized at times with both red and yellow on the same crystal. Nobody knew whether the bicolored salt was any less satisfactory than a uniformly colored product; but it did not meet the specifications. From the point of view of the War Department the work of the group was eminently successful, because rules were laid down which prevented the formation of parti-colored crystals. From a scientific view-point the work of the group was inconceivably bad. When we finished, we knew less about the theory of the subject than when we started because we had apparently eliminated all possible explanations. That merely meant bad planning and faulty experimental work. I have always looked upon the particular investigation as a model of what scientific research should not be, although there are people who point with pride to it.

This sort of thing is not uncommon. There has been a lot of work done on the action of radium and of ultraviolet light on gems, and the net theoretical outcome is practically zero.[1] "When we come to the color of gems, we get into an absolutely unknown country. In many cases a reversible change is possible. Heat makes the gem colorless, ultraviolet gives it one color, and radium (usually beta and gamma rays) give it another color.[2] Colorless topaz is made yellow to orange by radium and heating makes it colorless again. Ultraviolet light tends to change the radium orange to lilac. Kunzite changes from lilac to green, under the action of radium, while ultraviolet light reverses this change.[1] Heating makes it colorless and radium restores the green color. This cycle[2] can be repeated indefinitely. Blue sapphires are changed to yellow by radium and yellow sapphires to blue by ultraviolet light. Sapphires become colorless when heated and white sapphires are turned yellow by radium. Newbery and Lupton[3] report that all the colored fluorspars are decolorized when heated. Radium restored the green to a decolorized green crystal but changed a decolorized yellow one first to blue and then to purple. Garnett[4] quotes Soddy to the effect that colorless gold glass is turned ruby color by radium emanation, and Ramsay to the effect that colorless silver glass is turned yellow under these conditions; but Lind was not able to duplicate these results, perhaps because he used a different glass. Lind found that a yellow diamond was turned green by alpha rays and went back to yellow when heated. The green may have been due to a production of blue which disappeared on heating, the yellow being permanent. The ruby becomes green when heated and red again on cooling; this is not a case of selective emission at higher temperatures.

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[1] Bancroft: "Applied Colloid Chemistry," 254 (1926).

[2] Cf. Meyer and Przibram: Z. physik. Chem., *100,* 344 (1922).

"We do not know to what extent the color changes are due to a change in the size of the particles or to a chemical change. Cathode rays set free metallic sodium from sodium chloride crystals. The wise thing would be to start with the oxides of copper, manganese, chromium, and iron in a borax bead and study the color changes there before trying to straighten out the behavior of gems."

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[1] This change is undoubtedly due to chromic oxide.

[2] Lind and Bardwell: J. Franklin Inst., *196,* 375, 521 (1923).

[3] Memoirs Manchester Phil. Soc., *62,* No. 10 (1918).

[4] Phil. Trans., *203* A, 400 (1904).

Since this was written, we have shown that the color of the ruby is due to a red modification of chromium oxide, that the amethyst color produced by manganese is due to manganic oxide, Mn_2O_3, and that the green color obtained with copper oxide in lead glazes is due to the unsuspected presence of cuprous oxide. When we shall have cleared up the colors due to the oxides of titanium and iron, we shall be in a position to study the effect of radium and of ultra-violet light on beads containing these oxides, after which it will undoubtedly be a simple matter to account for the behavior of gems. This will illustrate the advantage of a flank attack. Hitherto, people have merely collected data and have not planned their work at all.

Plotnikow[1] says that "the amounts of substance changed in the whole region of photochemical absorption is proportional to the absorbed quantity of light, independent of the wave-length." So far as one can judge, this relation cannot be true if the nature of the reaction changes with the wave-length of the photochemically active light. If so, this is tantamount to saying that the nature of the reaction is independent of the wave-length of the photochemically active light. H. S. Taylor also believes that light can produce no localized increment of energy and that the molecules will break at the weakest points regardless of where the light enters. One would have supposed that a question like this, which is fundamental, would have been tested carefully; but the ordinary thinking is so inefficient that there is very little evidence either way and what little evidence there is about balances.

H. S. Taylor thinks that one of the mercury lines decomposes formic acid in two ways, into carbon monoxide and water and into carbon dioxide and hydrogen. As far as it goes, that is pretty conclusive that monochromatic light may act in two ways, though one does not quite see why there should be two weakest points. As against this we have the experiments of Berthelot and Gaudechon[1] that light with a wave-length longer than $300\mu\mu$ decomposes acetaldehyde according to the equation $CH_3CHO = CH_4 + CO$, while light of wave-length about $250\mu\mu$ polymerizes acetaldehyde to para-aldehyde and metaldehyde. These experiments have not been confirmed independently; but we are starting on this at Cornell.

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[1] "Lehrbuch der Photochemie," 55 (1920).

Destructive criticism is often necessary; but it is not the last word. One must eventually present something constructive. The answer is to be found in the game of twenty questions. When I was a small boy, it was a very popular game to try to find, by asking a series of questions, what the others had selected. The first question was always: animal, vegetable or mineral? After that the questions must be ones that could be answered by yes or no. It is astonishing what things one can find out in this way, if one is not limited to twenty questions. The trick was to frame the questions so as to eliminate a large portion of the possible field each time and to reach the goal by successive eliminations. Direct guesses were usually a waste of time and were not made by the experts. The method might be called the either–or method, or the Socratic method. The latter sounds more impressive. That simple little game exemplifies the principles of scientific research and it would be a good thing if our graduate students would play it regularly as part of their research training.

We make use of the principle involved whenever we weigh anything on a balance in the laboratory. The student is taught to get a weight that is too low and one that is too high and then to reach the true weight by bringing those two limits closer and closer together. If he guesses at the true weight and then keeps adding weights, he can waste a great deal of time.

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[1] Compt. rend., *156*, 68, 233 (1913).

Modern naval gunnery is based on the Socratic method or on the game of twenty questions as one prefers. As I understand it, nobody tries to hit the target on the first two shots. One shot is put over the target and the other under. Once a bracket is established, it is a relatively simple matter to hit a target on the third shot.

The advantages of the Socratic method are that one eliminates a great many possibilities definitely, that there is no error due to changing two variables simultaneously, and that one knows exactly where one has failed if that has happened. That means that the problem can be taken up again at some later time whenever one sees what the next step is. The sources of error are: inadequate exclusiveness; experimental error; inability to devise an experiment to enable one to distinguish between the mutually exclusive alternatives; inability to formulate an either-or.

It is not easy to illustrate the application of the Socratic method from the literature because the people who have employed it usually do not give the mental processes through which they have gone and one forgets them in one's own case. One very striking instance has been mentioned in the second lecture to illustrate another point. Sir Humphry Davy believed that the electrolysis of pure water should not give acid and alkali at the poles. When he actually obtained acid and alkali from what he had hoped was pure water, he proceeded to run down the sources of error in a way which is a model to all research men. Davy electrolyzed distilled water between gold wires in glass vessels connected by animal bladder or by moist strings. When the result was unsatisfactory Davy reasoned that the disturbing factors either came from the connecting fibers or did not, so he replaced them by well-washed asbestos. He next concluded that glass was either a disturbing factor or not, and he replaced the glass by agate vessels. Part of the disturbance was either due to the agate or it was not, so he replaced the agate vessels advantageously by gold cones. He had now apparently eliminated everything except the water, so he gave that a special distillation and eliminated all acid. His final conclusion was that the disturbance was either due to the air or it was not. Working with his specially distilled water in gold cones with asbestos connecting fibers in an atmosphere of hydrogen, he succeeded in electrolyzing water without the production either of acid or of alkali.

After looking up all the literature on solarization—the reversal of the photographic image—and before making any experiments, I wrote down three hypotheses which seemed to me to cover the field. The first and most plausible one, so far as the available data were concerned, was that the phenomenon was due to an oxidizing action of the light. This was easily proved wrong because the solarization phenomena could be duplicated by the action of suitable reducing agents on the photographic film. I do not remember now what the third tentative explanation was, because the second one proved to be right, that the effect is due to different rates of development.

The evidence in regard to the nature of the latent image in photography can be presented in a similar manner.[1] "Owing to the very slight change in silver bromide on short exposure, it has always been popular to assume that the latent image is a physical or allotropic modification. Namias[1] assumed polymerization, Hurter and Driffield[2] depolymerization, Bredig[3] mechanical disintegration, Chapman Jones[4] labile form, and Bose[5] a mechanical strain. All these assumptions, and the further one of von Tugolessow[6] that the latent image is an oxidation product, were overthrown by the simple fact that all the phenomena of the latent image can be duplicated by immersing the plate in a solution of a weak reducing agent,[7] such as sodium arsenite. This proves that the latent image is some reduction product of silver bromide. It cannot be a single, definite subhalide because no such compound has been prepared, because no satisfactory chemical reactions can be assigned to it, because the prolonged action of light does not yield the pure compound, and because this hypothesis cannot be reconciled with the facts of solarization.

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[1] Bancroft: Trans. Faraday Soc., *19,* 249 (1923).

"The latent image cannot consist of a number of definite subhalides because we cannot isolate these in any way or give any proof of their existence or properties, and because it is absurd to assume a number of definite subhalides between pure silver bromide and silver bromide containing an excess of 0.5 per cent silver, when one can account for all the facts much better on the assumption of a phase of continuously varying composition.

"The latent image cannot be free metallic silver (nucleus theory) because it does not show the chemical reactions of free metallic silver, because it does not show the electrical potential of free metallic silver, and because the hypothesis cannot be reconciled with the facts of solarization.

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[1] "Chimie photographique," 102, 110 (1902).

[2] Phot. J., *22,* 149 (1898).

[3] Eder: Jahrbuch der Photographie, *13,* 365 (1899).

[4] "Science and Practice of Photography," 383 (1904).

[5] Phot. J., *26,* 146 (1902).

[6] Phot. Correspondenz, *40,* 594 (1903).

[7] Bancroft: J. Phys. Chem., *14,* 294; Perley: 689 (1910); Clark: British J. Photography, *69,* 462 (1922).

"The latent image is due to silver adsorbed by silver bromide because it behaves like a phase of continuously varying composition, because this hypothesis enables us to account for all the chemical reactions, and because this hypothesis enables us to account for the facts of solarization. The first suggestion that the latent image is due to adsorption was made by Carey Lea,[1] but this came before people were ready for it, and the idea was really carried through by Lüppo-Cramer.[2] . . . Hartung[3] has shown experimentally that thin films of silver chloride, bromide, and iodide lose weight when exposed in air to the sunlight. This is due to loss of halogen, and the original weight is restored almost completely by rehalogenation. As was to be expected, the decomposition takes place much more rapidly in a vacuum."

The application of the Socratic method has enabled us to make real progress[4] in the study of the action of salt solution on gelatine. Gelatine is peptized by a potassium iodide solution much more readily than by a potassium chloride solution. It is a typical case of the Hofmeister series. Either the peptization is due to strong adsorption of the iodide ion or it is not. Nobody has been able to show any marked adsorption of the iodide ion and the effect cannot be due to this because the order of peptization by iodides and chlorides does not reverse as we change from a slightly acid to a slightly alkaline solution. Excluding adsorption also excludes compound formation.

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[1] Am. J. Sci., (3) *33,* 349 (1887).

[2] Cf. "Kolloidchemie and Photographie," 70 (1908).

[3] J. Chem. Soc., *121,* 682 (1922).

[4] Bancroft: J. Phys. Chem., *30,* 1194 (1926).

The effect is either due to the solvent action of potassium iodide or it is not. We cannot be dealing with a solvent action of liquefied potassium iodide because the gelatine is peptized and not dissolved. It cannot be a peptization by liquefied potassium iodide because that would mean a selective adsorption of iodide which we do not have. We may assume that the effect is due to hydration of potassium iodide or is not. So long as people believe that potassium iodide is more hydrated than potassium chloride, this cannot be true because the formation in solution of hydrates of potassium iodide would decrease the amount of free water and would therefore decrease the peptizing action of the solution of gelatine.

Since we have apparently excluded any direct action on the gelatine, the only remaining possibility is that potassium iodide changes the water so as to increase the water's peptizing action on gelatine. This is the more plausible because the Hofmeister series for chloride, bromide, and iodide crops up in one form or another as affecting all sorts of phenomena from the apparent hydrogen ion concentration to the temperature of maximum density of water. The mechanism of the effect is easy to see. Everybody considers liquid as an associated liquid with a reversible equilibrium between hydrol (H_2O), dihydrol $(H_2O)_2$, trihydrol $(H_2O)_3$, and perhaps polyhydrol $(H_2O)_n$. Sutherland considers that at zero liquid water consists practically of 37 per cent trihydrol and 63 per cent dihydrol.

The addition of a salt will displace the water equilibrium, whatever it may be, in the direction of the form in which the salt is more soluble. Since the two forms will not necessarily have the same peptizing action on gelatine, displacement of the water equilibrium may increase or decrease the peptizing action on gelatine. Since potassium chloride, bromide, and iodide increase the lowering of the temperature of maximum density of water, we conclude for the present that these salts increase the percentage of dihydrol, and that dihydrol has a greater peptizing action on gelatine than trihydrol has.

This could not have been predicted in advance and we know one case where the polymerized liquid is the active substance. Pyroxylin is peptized by an ether-alcohol mixture and not by either pure liquid at room temperature. Ethyl alcohol causes pyroxylin to swell appreciably, while ethyl ether does not. It has been shown by Lunge[1] and confirmed by Byron[2] in the Cornell laboratory that the ether apparently plays no direct part in the peptization of pyroxylin. It must therefore activate the alcohol either by increasing or decreasing the relative amount of depolymerized alcohol. One would expect a dilution of the alcohol to increase the relative amount of depolymerized alcohol; but this is apparently not the case. If the depolymerized alcohol is the peptizing agent, it should be possible to peptize pyroxylin by alcohol alone at higher temperatures, as one can do with gelatin and water. If the polymerized alcohol is the peptizing agent, it should be possible to peptize pyroxylin by alcohol alone at lower temperatures. It has been shown by McBain[3] and confirmed by Byron that there is no appreciable peptization of pyroxylin by alcohol at 140°. It has been shown by Kugelmass[4] and by McBain, and confirmed by Byron, that pyroxylin is peptized by alcohol at very low temperatures. If the hypothesis is right, a colloidal solution prepared at low temperatures should become more viscous as the temperature rises, because the relative amount of the peptizing agent decreases with rising temperature. It has been shown by McBain and confirmed by Byron that a colloidal solution of pyroxylin in alcohol, which is quite mobile at low temperatures, becomes a soft flowing jelly at room temperatures. It has been shown by Byron that this soft flowing mass becomes a stiff jelly when heated well above 100°, and that the tube can be turned upside down without the contents flowing out.

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[1] Z. angew. Chem., *14,* 538 (1901).

[2] J. Phys. Chem., *30,* 1116 (1926).

[3] McBain, Harvey and Smith: J. Phys. Chem., *30,* 312 (1926).

[4] Rec. Trav. chim., *41,* 751 (1922).

While we have no independent confirmation as yet of the assumption that ether increases the polymerization of ethyl alcohol, Centnerzwer and Zoppi[1] have shown that "ether causes great polymerization of methyl alcohol."

One of the Cornell non-resident lecturers in chemistry, Professor Barger of the University of Edinburgh, said in one of his lectures that he could not guess how Steenbock got the happy idea of irradiating food to increase the vitamin D content. I do not know Steenbock; but I know how his mind ought to have worked and, therefore, how it probably did work. It was known that irradiating people was equivalent to increasing the amount of Vitamin D in the food. This might be due either to the action of light on some actual constituent of the food or to the action of light on some decomposition product of the food. The way to distinguish between these two hypotheses was to try the action of light on the food itself. While Steenbock's discovery may have been a happy accident, I prefer to believe, and shall believe until I am proved wrong, that Steenbock's discovery was the result of first-class thinking along proper lines.

It seems to be quite clear that the Socratic method, as defined, is the proper way to do conscious, scientific thinking, and that graduate students should practise themselves in this as much as possible. When we consider unconscious thinking we come into a field in which most of the psychologists are quite definitely on one side and most of the research men in other sciences equally definitely on the other side.

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[1] Z. physik. Chem., *54*, 700 (1906).

It seems to be generally recognized that scientific ideas seem often to be an inspiration. Whewell[1] says that "scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin—some fortunate cast of intellect rising above all rules. No maxims can be given which will inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius."

Libby[2] says that "it will be found that scientific discovery, while predominantly an intellectual process, varies with the nature of the phenomena of the different sciences and the individual mental differences of the discoverers. As stated at the outset, the psychology of scientific discovery must be the subject of prolonged investigation, but some data are already available. One great mathematician, Poincaré, attributes his discoveries to intuition. The essential idea comes with a sense of illumination. It is characterized by suddenness, conciseness, and immediate certainty. It may come unheralded, as he is crossing the street, walking on the cliffs, or stepping into a carriage. There may have intervened a considerable time free from conscious effort on the special question involved in the discovery. Poincaré is inclined to account for these sudden solutions of theoretical difficulties on the assumption of long periods of previous unconscious work.

"There are many such records from men of genius. At the moment the inventor obtains the solution of his problem his mind may seem to be least engaged with it. The long-sought-for idea comes like an inspiration,[1] something freely imparted rather than voluntarily acquired. No mental process is more worthy of common respect; but it may not lie beyond the possibility of explanation. Like ethical insight, or spiritual illumination, the scientific idea comes to those who have striven for it. The door may open after we have ceased to knock, or the response come when we have forgotten that we have sent a call; but the discovery comes only after conscious work. The whole history of science shows that it is to the worker that the inspiration comes, and that new ideas develop from old ideas."

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[1] "Novum Organum Renovatum," 44 (1858).

[2] "An Introduction to the History of Science," 267 (1917).

Westaway[2] has an interesting page on the intuitions of great minds. "There are times in men's thinking when great truths seem to dawn upon the mind. On such occasions there is commonly not only simple intuition, but also the gathered wisdom of long and varied and ripened experience, refined analysis and generalization, conscious reasoning, exceptional talent, and systematic methods of working. In nearly every great discovery there has been a combination of native gift, accumulated experience, and connected reasoning; at some particular moment a truth has flashed upon the vision as if light from many sources were suddenly focussed on the same point.

"In science and mathematics, intuition is recognized as a legitimate mode of discovering truth. In the integral calculus, for instance, numerous results have been arrived at intuitively, and the clue to all mathematical problems is obtained by an intuition or insight which is due partly to native capacity and partly to knowledge and previous experience. As to the fruitful hypotheses that have led so often to verifiable results in natural science, they seem to be almost of the nature of inspired guessing: truth seems to flash across the mind of the inquirer immersed in his research. He becomes aware of something of which a moment before he was not aware. He is not conscious of having arrived at it by any process of logical thinking, but it has dawned upon him. Great scientific discoverers are men who appear to possess almost a genius for the intuition of hitherto unknown facts. So suddenly does the intuition come, without any conscious process of deductive or inductive reasoning, that it is sometimes regarded as a supra-rational faculty. For the moment it seems to be more akin to imagination than to logic, being creative and spontaneous, apparently independent of the mental processes of analysis and synthesis which constitute the ordinary machinery of thought. But in such cases reasoning and intuition are probably always complementary processes; alone, neither is sufficient. Logic does not discover the data with which it works; the premises of a syllogism must be known before the inference can be drawn. Hence intuition is necessary. But, as we have seen, intuition is very largely the result of garnered experience, and this is brought suddenly into instantaneous action. But inasmuch as the experience is necessarily affected with error, the intuitions should always be scrutinised and put to the test of truth. Reason should always be called in, even though it be but to crystallise the findings of intuition.

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[1] [Cf. for instance, Helmholtz in Ostwald: "Grosse Männer," 302 (1909).]

[2] "Science and Theology," 251 (1920).

"Just as we talk of the mind being suddenly illuminated by a sudden idea, so we talk of the suddenness of a flash of lightning. We are apt to forget that every flash of lightning is the direct result of natural forces gradually but inevitably leading up to it, and could, if knowledge had been sufficient, have been foretold in the remote past. Things are sudden only because we do not foresee them, and their suddenness is no inherent quality in themselves but the mere result of our ignorance. In reality, nothing is more sudden than anything else. It is from our lack of knowledge, understanding, and preparation that lightning borrows its suddenness.

"All this seems to be equally true in the world of the mind. To the sudden idea long thinking has contributed—thinking which we now seem to be unconscious of. In our unconsciousness seem to lie some of the greater powers we possess. Not a little of man's capital work is done without his knowing it, and when it is done he is amazed at the apparent suddenness of results that reveal themselves in their maturity."

Even when we make large allowance for the enthusiasm of biographers and writers of popular science, there can be no doubt about the suddenness with which ideas come. Koch[1] was trying to get the anthrax bacilli pure. "Then one day a perfectly easy, a foolishly simple way to watch his rods grow flashed into Koch's head." More striking is what deKruif says about Metchnikoff:[2] "Metchnikoff sat alone in his parlor, tugging at his biblical beard, gazing without seeing them at his bowls of star fish. Then—it was like the blinding light that bowled Paul over on his way to Damascus[3]—in one moment, in the most fantastical you would say impossible, flash of a second, Metchnikoff changed his whole career." As Metchnikoff wrote in his diary, "I suddenly became a pathologist. . . . Feeling that there was in this idea something of surpassing interest, I became so excited that I began striding up and down the room, and even went to the seashore to collect my thoughts."

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[1] DeKruif: "The Microbe Hunters," 113 (1926).

[2] p. 212.

[3] [The same phrase is used in regard to Julius Robert Mayer and the laws of conservation of energy. Ostwald: "Grosse Männer," 65 (1909).]

Although Jastrow[1] does not himself believe in inspiration, he records some instances where this seems to occur. "Among recorded instances of important discoveries emerging into consciousness at such indirect moments of leisurely occupation, 'when the mind is at lullaby,' I have noted the following:—

"Sir William Rowan Hamilton evolved the intricate conception of the invention of quaternions while walking with Lady Hamilton in the streets of Dublin, the flash of discovery coming to him just as he was approaching the Brougham Bridge. Mozart had the aria of the beautiful quartette in the 'Magic Flute' come to him while playing a game of billiards, and seemed prepared for such occasional influxes of musical ideas by carrying a note-book for their instant record. An inventor suddenly conceived the proper way of constructing a prism for a binocular microscope—a problem which he had long thought of and abandoned—while reading an uninteresting novel. Professor Kekulé tells how he saw the atoms dancing about in mid-air in conformity with his theory of atomic grouping, while riding on top of a London bus. In the attempt to recall a name that is on the tip of the tongue many persons deliberately occupy themselves with something irrelevant, finding by experience that this is an aid; and the day-dream through which flashes a happy 'Eureka,' or the dream of deeper sleep that discovers the treasures that our laborious digging has failed to unearth, are equally instances in which the fixed intent of the more watchful consciousness is withdrawn."

In another place[2] Jastrow says: "Without maintaining that the extremely variable, even discordant, descriptions recorded by inventors, artists, composers, authors, and others in regard to the genius of their several pursuits, at all supply what the psychologist is interested in discovering, it may none the less be profitable to consider one such account—that of Robert Louis Stevenson—for the suggestiveness of the matter which it so attractively presents. It pleases this master of imaginative construction to speak of the moments of inspiration as coming to him in dreams,—waking as well as sleeping dreams, we may assume,—and the subconscious contributors to his inventions are made to appear as Brownies.

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[1] "The Subconscious," 94 (1906).

[2] Ibid., p. 70.

"This dreamer (like many other persons) has encountered some trifling vicissitudes of fortune. When the bank begins to send letters and the butcher to linger at the back gate, he sets to belaboring his brains after a story, for that is his readiest money-winner; and behold! at once the little people begin to bestir themselves in the same quest, and labor all night long, and all night long set before him truncheons of tales upon their lighted theatre. No fear of his being frightened now; the flying heart and the frozen scalp are things bygone; applause, growing applause, growing interest, growing exaltation in his own cleverness (for he takes all the credit), and at last a jubilant leap to wakefulness, with the cry 'I have it, that'll do!' upon his lips: with such and similar emotions he sits at these nocturnal dreams, with such outbreaks, like Claudius in the play, he scatters the performance in the midst. Often enough the waking is a disappointment; he has been too deep asleep, as I explain the thing; drowsiness has gained his little people, they have gone stumbling and maundering through their parts; and the play, to the awakened mind, is seen to be a tissue of absurdities. And yet how often have these sleepless Brownies done him honest service, and given him, as he sat idly taking his pleasure in the boxes, better tales than he could fashion for himself.

"In spite of this poetical transformation, the psychological affinities are recognizable, though fancifully disguised; and when the dreamer comes to hold an accounting for the share of his intent and his prompted self in the joint contract, he apportions the credit quite fairly. The Brownies 'are near connections of the dreamer's, beyond doubt; they share in his financial worries and have an eye to the bank book; they share plainly in his training; they have plainly learned like him to build the scheme of a considerable story and to arrange emotion in progressive order; only I think they have more talent; and one thing is beyond doubt, they can tell him a story piece by piece, like a serial, and keep him all the while in ignorance of where they aim.' They 'do one half my work while I am asleep, and in all human likelihood do the rest for me as well, when I am wide awake and fondly suppose I do it for myself.' And though this sensitive writer is tempted to suppose that his conscious ego 'is no storyteller at all, but a creature as matter of fact as any cheesemonger or any cheese, and a realist bemired up to his ears in actuality,' the reader cannot share this doubt. Stevenson comes again upon psychological ground when he says: 'I am an excellent adviser, something like Molière's servant; I pull back and I cut down; and I dress the whole in the best words and sentences that I can find and make; I hold the pen, too; and I do the sitting at the table, which is about the worst of it; and when all is done, I make up the manuscript and pay for the registration; so that on the whole, I have some claim to share, though not so largely as I do, in the profits of our common enterprise.'

"Dr. Holmes similarly acknowledges the portion of our subconscious acquisitions in our successes, and tells us that 'we are all more or less improvisators; we all have a double, who is wiser and better than we are, and who puts thoughts into our heads, and words into our mouths,' yet equally does he realize that the inspiring source of these subconscious thoughts is really the conscious 'grinding' self."

There is a very wide-spread recognition among people who are not psychologists of the possible importance of the subconscious mind. Dr. Oliver Wendell Holmes speaks of those subconscious products of our intelligence wrought in the underground workshop of thought. He also says: "We wish to remember something in the course of conversation. No effort of the will can reach it; but we say, 'Wait a minute and it will come to me,' and go on talking. Presently, perhaps some minutes later, the idea we are in search of comes all at once into the mind, delivered like a prepaid bundle, laid at the door of consciousness like a foundling in a basket. How it came there we know not. The mind must have been at work groping and feeling for it in the dark; it cannot have come of itself. Yet all the while, our consciousness, so far as we are conscious of our consciousness, was busy with other thoughts."

In "The Three Hostages" by John Buchan, the doctor says: "I belong to more or less the same totem as you, but I've long been aware that I possessed a most curious kind of subconsciousness. I've a good memory and fair powers of observation, but they're nothing to those of my subconscious self. Take any daily incident, I see and hear, say about a twentieth part of the details and remember about a hundredth part—that is, assuming that there is nothing special to stimulate my interest. But my subconscious self sees and hears practically everything and remembers most of it. Only I can't use the memory, for I don't know that I've got it, and can't call it into being when I wish. But every now and then something happens to turn on the tap of the subconscious, and a thin trickle comes through. I find myself sometimes remembering names I was never aware of having heard, and little incidents and details I had never consciously noticed. Imagination, you will say, but it isn't, for everything that that inner memory provides is exactly true. I've tested it. If I could find some way of tapping it at will, I should become the first scientist of the age, for the trouble with investigation and experiment is that the ordinary brain does not observe sufficiently keenly or remember the data sufficiently accurately."

Since Mr. Buchan is not primarily a research man, one cannot hold it against him that he believes in the Baconian method of research—collecting data and having the generalizations become self-evident. The important thing is that the existence of what he calls the subconscious self is perfectly obvious to him and is not a speculative hypothesis.

We get exactly the same thing in a slightly different form from John St. Loe Strachey in "The Adventure of Living." "There is another point in regard to Senior's power of recording conversations which is worth considering by modern psychologists. It is quite possible that what Senior did, unconsciously of course, was to trust to his subconsciousness. That amiable and highly impressionable, if dumb, spirit which sits within us all got busy when Thiers or Guizot was talking. The difficulty was to get out of him what he had heard and had at once transferred to the files in the Memory Cupboard. Senior, without knowing it, had, I doubt not, some little trick which enabled him to get easily *en rapport* with his subconsciousness, and so tap the rich and recently stored vintage. His writing was probably half automatic. It certainly was vivid and dramatic in a high degree."

Emerson's phraseology[1] is quite different of course; but he is also speaking about a thing that is real to him. "Our spontaneous action is always the best. You cannot with your best deliberation and heed come as close to any question as your spontaneous glance shall bring you, while you rise from your bed, or walk around in the morning after meditating the matter before sleep on the previous night. Always our thinking is a pious reception. Our truth of thought is therefore vitiated as much by too violent direction given by our will, as by too great negligence."

"It is a secret which every intellectual man quickly learns, that beyond the energy of his possessed and conscious intellect he is capable of a new energy (as of an intellect doubled on itself), by abandonment to the nature of things; that, besides his privacy of power as an individual man, there is a great public power on which he can draw, by unlocking, at all risks, his human doors, and suffering the ethereal tides to roll and circulate through him: then he is caught up into the life of the Universe, his speech is thunder, his thought is law, and his words are universally intelligible as the plants and animals. The poet knows that he speaks adequately, then, only when he speaks somewhat wildly, or 'with the flower of the mind'; not with the intellect used as organ, but with the intellect released from all service, and suffered to take its direction from its celestial life; or, as the ancients were wont to express themselves, not with intellect alone, but with the intellect inebriated by nectar. As the traveller who has lost his way throws his reins on his horse's neck, and trusts to the instinct of the animal to find his road, so must we do with the divine animal who carried us through this world. For if in any manner we can stimulate this instinct, new passages are opened for us into nature, the mind flows into and through things hardest and highest, and the metamorphosis is possible."

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[1] "Essays," *5*, 232, 279 (1906).

There is no doubt as to what Buchan and Strachey are trying to say; but one can read almost anything into Emerson's words. I think that the main difference is that Buchan and Strachey consider that what one calls inspiration comes from within, whereas Emerson implies that one opens the windows of his mind and lets the inspiration blow in from outside. Emerson seems to eliminate the subconscious self thereby; but it is a little obscure whether the "divine horse" is internal or external.

In an address given ten years ago, Graham Lusk[1] discussed the question of research in medicine. "Helmholtz, on the occasion of his seventieth birthday, stated that he had never had a great idea come to him when he was at his desk, nor when he was tired, nor after taking a glass of wine; but usually such had come to him when he was walking in the garden musing of other things. The scientist must have leisure to think over the problems which offer and he must have a certain discrimination in order to distinguish between the things that are worth doing and those which are not. To do this requires a certain delay in action in order that plans may be matured. The individual who can not be happy unless he is at work at full power all the time is much less likely to accomplish successful scientific work than he who will not commence a research until he has satisfied himself that it is worth doing. It is not to be denied that this essential qualification of scientific life is frequently regarded with scorn by the busy practitioner of medicine who gives himself no time either for thought or discovery."

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[1] Science (2) *48,* 634 (1918).

The ordinary man, in trying to describe his experiences, usually speaks of a subconscious self or a subliminal self; but both of these terms are anathema to the psychologists, partly because these latter mean something different by these terms from what other scientific men mean. Jastrow[1] furnishes a striking instance of this.

"While confining the exposition to what is offered as the most convincing interpretation, it is well to appreciate the attitude of a differently derived and maintained survey. An opposite theory has framed its conception upon a fundamental emphasis of the schism of conflicting personalities, and upon the exceptional nature of allied phenomena. To account for these, it supposes the existence in the mental constitution from the outset and in all its phases, of a factor wholly different from any here recognized, a pervasive influence in the psychic organism that only in exceptional circumstances becomes articulate, and is thus hampered in its expression, because until released from the thrall of ordinary consciousness, it cannot throw off its enforced silence. It awaits the rare conjunction of circumstances and temperament, and then shoots forth in spontaneous perfection. It reaches independent expression in the emergence of a new personality, in the exaltations of trance, in the superior susceptibilities of hypnosis, in the inspirations of genius, in the peculiar endowments of gifted souls. The issue may be most tangibly presented when applied to the interpretation of the calculating prodigies, whose performances certainly exhibit a more than ordinary development of some type of subconscious facility. In giving name to the theory in question, let it be the designation in common use among its adherents: that of the subliminal self. It admits that a decided proficiency in rapid calculation may be developed upon the basis of intensive cultivation and natural talent, and that performance so achieved may indeed be notable; but it regards certain of these performances as not thus applicable, but as evidence of a wholly different mode of procedure. It points out that the performers are often boys of no high order of general intelligence, whose own accounts of their training and methods contain no adequate basis for such extreme facility, and who, indeed, regard themselves as the receptive instruments of a faculty that is somehow exercised through the agency of their mind, which passively receives the solutions as a revelation.

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[1] "The Subconscious," 532, 537 (1906).

"While admitting, as applied to our general proficiencies, that much of our intercourse is replete with short-circuiting processes, that our notes of experience are recorded in a mixture of long-hand and short-hand characters, in the interpretation of which we have acquired a facile talent, the view in question none the less holds that in unusual cases, characters appear that are not stenographic records of ordinary experience at all, but are of an independent alphabet, and bear a message removed from the ken of the mind that is ordinarily directive.

"In development of this conception, the theory discovers in hypnosis the exercise of a power by which is thus subliminally revealed knowledge that has no origin in the experiences open to the self that responds to the ordinary vicissitudes of life. It regards hallucinations as of the same status, and attempts to determine their import not from inner analysis, but from the detailed conformity of their content to objective fact, at times in anticipation of the future, at times in overcoming temporal and special limitations. The exalted sensibilities of hysteria are similarly appraised; and alterations of personality become the most explicit expression of a release of the confined subliminal self, whose experiences, though seemingly trivial and chaotic, and for the most part admittedly decadent, are akin to the recondite sources from which, by a different use of a common privilege, the exceptional man of genius draws his inspiration. The very latitude of this theory makes it hospitable to a wide range of considerations,—many of them supported by questionable data and strained interpretations,—and renders it liable to affiliation with 'occult' conceptions of every shade and grade of extravagance. This 'tumbling ground for whimsies,' in Professor James' phrase, there is no obligation to inspect. It is proper to direct attention to the serious shortcomings of the theory of the subliminal self, when most conservatively framed and when applied in the spirit of psychology, not of a plea for the supernatural.

"To begin with it seems difficult to understand how such an independent, and in its essence transcendent capacity could have found maintenance in the evolutionary conditions of our being. To conceive it as an atavistic function that is in its decadence is clearly unnatural, because such functions can hardly be concerned with the economies of elaborated and highly complex service; atavism is survival from below, not a culling from above. It can only be urged that consciousness is itself a lapsed function, adjusted to the present stages of evolution, and has thus replaced a form of psychic energy that existed previous to consciousness, and achieved a perfection of mental efficiency similar, though superior, to that offered by our present form of that privilege; such issue was attained by service of susceptibilities now lost except in sporadic instances. Those who courageously embrace this view relieve themselves of further obligations to provide for subliminal functioning in normal life, and may be driven to this position by the difficulty of finding a place in the evolutionary field for a function of such occasional service and yet of such high potency and independent status. The feeble support that the conception finds when gauged by evolutionary standards is further disclosed in relation to the higher products of mental evolution. It seems a very mockery of that process to carry the development of the mind as the issue of tortuous and minute steps, laboriously and uncertainly attaining to its present stage of efficiency, and then to have these endowments and achievements outdone by a confined and untutored 'double,' that this same mind has all the while unwittingly nurtured. A complete parallel to such a supposition is not readily found; not wholly unlike it would be the assumption that the eyes were admittedly developed by virtue of their utility as organs of vision, but that somewhere in the bodily economy—say under a fold of the skin—there exists an organ that by a survived potency from primeval days can now, with suppressed experience or service, occasionally convey to the mind, when the eyes are closed or when a saving blindness releases the imprisoned sense, the same type of visions as come through the retina and yet more exalted ones. Until the conception can be better reconciled to evolutionary principles, it is highly improbable that it will find support by appeal to other logical considerations. The theory exposes its further shortcomings by a necessary admission of a different status for that large range of abnormal experience, presenting phenomena wholly parallel to those that it interprets in its own favor, but which are decidedly free from the features that require the assumption of the traits ascribed to the subliminal self. It is the less urgent to enforce these and related objections, for the reason that the theory, being but slightly restrained by exacting allegiance to the large body of normal data and by the systematic obligations thus incurred, has little difficulty in accommodating itself to the evasion of such objections by yet further complications of like hypothetical nature. The Copernicans were quite ready, when the observed positions of the planets departed from the predictions based upon the supposition of the circular orbit, to 'build, unbuild, contrive,' with 'cycle and epicycle, orb in orb'; the simplicity of the elliptical hypothesis of Kepler not alone did away with the cycles great and small, but rendered such questionable expedients unnecessary."

Even Dr. Morton Prince,[1] who believes in subconscious reasoning, draws the line at a subconscious or subliminal self outside of pathological cases. "Through the conception of the *sub*conscious as resolvable, on the one hand, into the *un*conscious, passive or active physiological dispositions, and, on the other hand, into *co*active conscious states, the subconscious becomes simplified and intelligible. It offers a basis on which may be constructed comprehensible theories of memory, suggestibility, post-hypnotic phenomena, dreams, automatic writing and similar phenomena, artificial hallucinations, the protean phenomena of hysteria, and the psychoneuroses, as well as the mechanism of thought. It enables, as well to construct a rational concept of personality and self. As we shall see, when we take up the study of multiple personality in later lectures, out of the aggregate of the accumulated and varied experience of the past conserved in the unconscious may be constructed a number of different personalities, each depending upon a synthesis and rearrangement of life's neurograms and innate dispositions and instincts. All dormant ideas with their feelings, tones and conative tendencies belong to our personality, but they may be arranged with varying instincts and innate dispositions into a number of differentiated systems, each synthesized into a corresponding personality. In the unconscious may be conserved a vast number of life's experiences ranging in time almost from the cradle to the grave. The hopes, the wishes, the anxieties of childhood may still be there, lying fallow, but capable of injecting themselves under favoring conditions into our personalities. Properly speaking, from this point of view, aside from certain artificial and pathological conditions, there is, *normally*, no distinct 'subconscious *self*,' or 'subliminal *self*,' or 'secondary *self*,' or 'hidden *self*.' In artificial and pathological conditions there may be, as has been frequently shown, a splitting of consciousness and the aggregation into a secondary coconscious system of large systems of ideas which have all the characteristics of personality. This secondary personality (of which the primary personality is not aware) may have its own memories, feelings, perceptions, and thoughts. It may appropriate to itself various complexes of neurograms deposited by the experiences of life which are not at the disposal of the principal personality. Such a coconscious system may properly be spoken of as a subconscious *self*. But there is no evidence that, *normally*, such systems exist. All that we are entitled to affirm is that every individual's consciousness may include ideas of which he is not aware, and that he has at his disposal, to a greater or less extent, a large unconscious storehouse in which are neurographically conserved a large and varied mass of life's experiences. These experiences may be arranged in systems, as we shall see in the next lecture, but they do not constitute a 'self.' To speak of them as a subconscious, subliminal, secondary, or hidden self is to construct concepts which are allegories, metaphors, symbolisms, personifications of concrete phenomena. Their use tends to fallacious reasoning and to perverted inductions from the facts. Becoming major premises in a syllogism they lead to erroneous interpretations of the simplest facts, just as fixed ideas or obsessions tend to a perverted interpretation of the environment."

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[1] "The Unconscious," 255 (1921).

Münsterberg says that "there is no subconscious." Titchener[1] says that "if we refuse to explain mind by body, we must accept the one of the two, equally unsatisfactory, alternatives: we must either rest content with a simple description of mental experience, or must invent an unconscious mind to give coherence and continuity to the conscious. Both courses have been tried. But, if we take the first, we never arrive at a science of psychology; and if we take the second, we voluntarily leave the sphere of fact for the sphere of fiction."

Woodworth[2] considers the question of the apparently unconscious solving of problems. "But now for the real 'subconscious mind.' You try to recall a familiar name, but are stuck; you drop the matter, and 'let your subconscious mind work'; and, sure enough, after a few minutes you have the name. Or, you are all tangled up in a difficult problem; you let the subconscious mind work on it overnight, and next morning it is perfectly clear. Just here it is that psychology begins to take issue with the popular idea. The popular interpretation is that work has been done on the problem during the interval when it was out of consciousness—unconscious mental work of a high order. But is it necessary to suppose that any work has been done on the problem during the interval? The difficulty, when you first attacked the problem, arose from false clues which, once they got you, held you by virtue of their 'recency value.' The matter laid aside, these false clues lost their recency value with lapse of time, so that when you took the matter up again you were free from their interference and had a good chance to go straight towards the goal."

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[1] "A Text-Book of Psychology," *1,* 40 (1909).

[2] "Psychology," 563 (1921).

This is an ingenious explanation of what might have happened; but, so far as I am concerned, it is in no sense a statement of what does happen.

Gault and Howard[1] object to certain specific uses of terms in regard to unconscious processes. "The psycho-analysts are responsible for having injected several new terms into the vocabulary of the psychologists *'subconscious mind'; 'subconscious'; 'subconscious experience'; 'the unconscious.'* These terms have been repugnant to psychologists of the most conservative sort—and naturally so. For them psychology has to do with consciousness; with conscious processes and nothing more. The unconscious is outside of their realm and 'subconscious mind' and 'subconscious experience' are contradictory in themselves. These last terms have all but simmered [?] out of existence over the heat of discussion pro and con; but the unconscious, in spite of philosophy and history, bids fair to prove non-volatile and to hold for itself a respectable place in the language of psychologists.

"The word has broad and varied connotations. Many nerve impulses do not arouse conscious processes for the reason that they do not reach the cortex; and yet they affect our behavior most usefully. . . . Many a student has gone to bed and fallen asleep after having vainly attempted to solve a mathematical problem, and has awakened in the morning with the solution all in order. Something had gone on in the meantime, albeit unconsciously. It was an unconscious process."

Troland[2] is not quite as pessimistic as some of the other psychologists, though he is not really very helpful. "Notwithstanding continued adversity, we need not abandon all hope of finding an intelligible conception of the mind as an agency underlying consciousness. We have still to examine a popular alternative of the *un*conscious, namely the '*sub*-conscious' mind. As the term suggests, the 'subconscious' is conceived as a true form of consciousness, which is nevertheless removed from the ordinary awareness of the individual, lying psychically 'under' the normal consciousness. The subconscious, in this sense, may possess any or all of the properties of the conscious realm, except continuity or unity with the latter. Just as my consciousness is separated from yours without either being *un*conscious or necessarily different in kind, so my subconscious may be cut off from my own consciousness and still be of the same general nature as the latter. Just as your consciousness may influence mine, so in even greater measure can my subconscious control the events in my conscious field.

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[1] "Outline of General Psychology," 407 (1925).

[2] "The Mystery of Mind," 26, 229 (1926).

"Here at last we have discovered a conception of mind which is truly intelligible. Although our idea of the 'subconscious mind' may be somewhat vague, it is nevertheless far from being a blank word like 'soul' or 'will.' However, we are not yet out of the woods. We know what we *mean* by the subconscious, but have we any proof that such a thing actually exists? Some psychologists feel very confident as to the reality of the subconscious, but others deny it without reservation. Nearly all would admit that the subconscious is an hypothesis rather than a fact; its existence cannot as yet be proven, but can only be rendered probable through its capacity to explain the actual facts. Accordingly if we identify the self with the subconscious we leave it as a problem whether the self really exists. That would be a rather unsatisfactory termination for our search."

"In its most significant aspect, consciousness corresponds to the *recording point* in the brain. It represents the 'moving finger' which writes upon the cortical surface. The central brain activity is a formative process. What it writes is determined to some degree by what has already been written at the place where the record is being made. We thus conceive of the central activity as wandering about over the association area, always leaving its impress behind it. The residual activities which continue on the basis of these impressions correspond with the subconscious mind. The wandering focus of activity corresponds with consciousness. The central activity very frequently retraces its movements or walks in some of its own footprints, thus bringing the material of the subconscious into consciousness, and combining the memory influence with that of the present forces of sensation.

"This view leads us to regard the subconscious on the one hand as the *product of consciousness,* and on the other hand as a vast *storehouse* of *materials* for the latter. Having once discovered these functions of the subconscious, we can substitute the latter for the brain in our explanations. Thus instead of explaining habit in terms of synaptic resistance, we can account for it in terms of some 'subconscious tendency.' Complexes regarded as nerve mechanisms will give way to emotional ideas, or the like, regarded as facts of the subconscious. The physiology of the cortex is to a large extent eliminated. This does not mean that we deny the existence of the cortical structures and activities, but only that we can get along without referring to them. Freud's account of his famous theory is given primarily in terms of a sentient subconscious rather than in terms of brain mechanisms."

This is one of the many cases in which the all-important thing is to know the special terms in vogue. While it is very bad form to speak of the subconscious or subliminal self, it is possible to convey the same ideas, without giving offense, by speaking[1] of being outside the margin of consciousness. To speak of the subliminal self is unpardonable. To speak of subconscious thinking is unwise. To speak of supra-marginal consciousness is to put oneself well out ahead of the procession.

"When speaking colloquially of the content of consciousness we have in mind those ideas of components of ideas—elements of thought—which are in the focus of attention, and therefore that of which we are more or less vividly aware. If you were asked to state what was in your mind at a given moment it is the vivid elements, upon which your attention was focused, that you would describe. But, as everyone knows, these do not constitute the whole field of consciousness at any given moment. Besides these there is in the background of the mind, outside of the focus, *a conscious margin or fringe* of varying extent (*consisting of sensations, perceptions, and even thoughts*) of which you are only dimly aware. It is a sort of twilight zone in which the contents are so slightly illuminated by awareness as to be scarcely recognizable. The contents of this zone are readily forgotten owing to their having been outside the focus of attention; but much can be recalled if an effort to do so (retrospection) is made immediately after any given moment's experience. Much can only be recalled by the use of the special technical methods of investigation. I believe that the more thoroughly this wonderful region is explored the richer it will be found to be in conscious elements.

"It must not be thought that, because we are only dimly aware of the contents of this twilight zone, therefore the individual elements lack definiteness and positive reality. To do so is to confuse the awareness of a certain something with that something itself. To so think would be like thinking that, because we do not distinctly recognize objects in the darkness, therefore they are but shadowy forms without substance. When, in states of abstraction or hypnosis, the ideas of this fringe of attention are recalled, as often is easily done, they are remembered as *very definite, real, conscious elements,* and the memory of them is as vivid as that of most thoughts. That these marginal ideas are not 'vivid' at the time of their occurrence means simply that they are not in such dynamic relations with the whole content of consciousness as to be the focus of awareness or attention. What sort of relations are requisite for 'awareness' is an unsolved problem. It seems to be a matter not only of synthesis but of dynamic relations within the synthesis."

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[1] Morton Prince: "The Unconscious," 340 (1921).

"Some of these elements may be so distinctly within the field of awareness that we are conscious of them, but dimly so.[1] Others, in particular cases at least, may be so far outside and hidden in the twilight obscurity that the subject is not even dimly aware of them. In more technical parlance, we may say, they are so far dissociated that they belong to *an ultra-marginal zone and are really subconscious.* Evidence of their having been present can only be obtained through memories recovered in hypnosis, abstraction, and by other methods. These may be properly termed coconscious. Undoubtedly the degree of awareness for marginal elements, i.e., the degree of dissociation between the elements of the content of consciousness, varies at different moments in the same individual according to the degree of concentration of attention and the character of the fixation, e.g., whether upon the environment or upon inner thoughts. It also varies much in different individuals. Therefore some persons lend themselves as more favorable subjects for the detection of marginal and ultramarginal states than others. Furthermore, according to certain evidence at hand, there is, in some persons at least, a constant shifting of interchange of elements going on between the field of attention and the marginal and the ultra-marginal zone—what is within the first at one moment is in the second, or is entirely subconscious, the next, and *vice versa*."

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[1] It is very doubtful whether vivid awareness is a matter of intensity because, among other reasons, subconscious ideas of which the individual is entirely unaware and elements in the fringe may have decided intensity.

"If all that I have said is true, it follows that the *whole content or field of consciousness at any given moment includes not only considerably more than that which is within the field of attention but more than is within the field of awareness*. The field of conscious states as a whole comprises the focus of attention plus the marginal fringe; and besides this there may be a true subconscious ultramarginal field comprising conscious states of which the personal consciousness is not even dimly aware."

Platt[1] is another man who delights in the term, margin. "It is that at any time we are using but a small portion of our available brain patterns. Only a small part of our mental store can be before us at any one moment of our existence. It is as with our eye vision, we see clearly only that upon which we focus. From this central point vision fades toward the margin where lie but the vaguest suggestions of sight, and here, at the margin, it finally ends. Beyond the margin, however, there lies, we know, a vast world not now seen at all, but which, piecemeal, can be brought into vision by changing the direction of our gaze. In our mental field, likewise, we see only that upon which we focus, that to which we give our attention. Beyond this appreciated field of our present consciousness lies the vastly greater field of unconsciousness, the field of the subconscious mind, where lie all the hundreds of thousands of experiences, brain patterns, and emotional potentialities which constitute our inheritance and our experience in life. 'There are secret and individual parts in the nature of men, and mute conditions without show, sometimes unknown of their very possessors."[1]

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[1] "The Psychology of Thought and Feeling," 157 (1921).

"But let us go a step further—our unseen world is not a dead world awaiting our regard before it awakens. It is with the mentally unseen world of the subconscious mind as it is with the vast world unseen by our eyes. This subconscious, unconscious mind of ours is by no means the quiet storehouse it was once assumed to be—deep down in the unconscious mind things happen. It is a storehouse, it is true, but not one of mere inactive waiting. There is going on in the unconscious mind, though possibly in modified form, the very same processes that are known to our consciousness. An unconscious cerebration is there taking place, a modified activity corresponding in kind to that we have already studied. We store away experiences, but these do not necessarily, nor generally, remain isolated; they may, under certain conditions, form unconscious associations and undergo change, and when again brought to consciousness may be altered beyond recognition. To the old orthodox attitude the above is absurd. In the old conception, with soul-controlled thought, the unconscious is simply something which is not. The poets and philosophers knew better. To them the unconscious has always been very real indeed, and so it is to the psychologist of today. The unconscious, it is believed, differs from the conscious merely in not being in focus, and in being incomparably greater and richer.

"We must add one concept more, namely this: that from the unconscious excursions take place into the conscious and have an effect on our conscious lives—that there is, in fact, a two-way connection between the conscious and the unconscious, a connection by no means always under the control of the will. Take a very simple example of this. You are seeking a name; you can not recall it; you finally abandon the quest and turn to something else. Suddenly the name comes to you—bobs up, as it were, from the unconscious into the conscious. While *you* were thinking of something else, the original mental process was contained in the unconscious, and there finally found the pattern sought. Or, more elaborately, we are striving to solve a problem, and can not, and give it up, and go to bed—and then wake in the morning with the answer clearly before us.[1]

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[1] Montaigne.

"Does not this unconscious cerebration, which, it would seem, must be accepted, endorse the description of thought given earlier? Our picture then was of a nerve force, the neurokyme, which once set in action flowed here and there until it found the brain pattern which would solve the problem presented.

"The fact that the unconscious sometimes succeeds better than our conscious effort is explainable, probably, by the presence in the latter of a wilful control which may be mistaken. Our conscious effort may be in the wrong direction, but we, nevertheless, obstinately hold to the path on which we have started. For example; we are trying to recall the name of a man, and we feel sure that it begins with a *B;* so we keep harping on Brown, and Baker, and Bissel, and Black—hanging on to the *B* idea at all costs. But when we finally give over the search, the *B* idea gets dropped also, and the subconscious, left to work it out by itself, finally finds what we want—the man's name was Adams. We remember now that we met him in Boston, and this is, of course, where the *B* idea came from.

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[1] "We sleep, but the loom of life never stops; and the pattern which was weaving when the sun went down is weaving when it comes up tomorrow." This from Henry Ward Beecher! Though we must acknowledge that what he had in mind was quite other than the psycho-physiological phenomenon we are considering.

"There are examples of simple unconscious cerebration and of recovery from the unconscious of facts lying, as it were, just beneath the surface. But this subconscious, unconscious region contains many, many other things than these temporarily laid aside items. In the unconscious lie all the memories, impressions, and experiences of life; events of our childhood, things near, and far past; things we have put away until needed; things we have tried to forget, and things we have forgotten. Here, too, are things we do not know and may never know we possess—passing impressions which may never have registered in the conscious mind, but which were, nevertheless, duly registered in the unconscious. Here, too, are our inheritances, our innate dispositions, our tendencies; some of them recognized by us at times; and some of them still but potentialities, their patterns never yet having been used, they never yet having received the stimulus necessary to set them in action. These last, we shall find, are important, for whether chance ever discovers them or not, they are there, and do surely affect, albeit unconsciously, our thoughts and our actions. Furthermore, being unsuspected, their control is neither sought nor obtained.

"We have reached now to a conception of a vast region of unconscious cerebration, from which arise impulses and influences which, whether they actually come to consciousness or not, do, still, profoundly affect our conscious lives. From this seething underworld of thought come many of the inhibitions, or *stops,* which obstruct our conscious acceptances, and from it, too, come many of our calls to action. We are influenced we know not, consciously, how nor why;

and we wonder at ourselves—it is the unconscious that is pulling!

"This conception of the unconscious is not new, it is only the precise statement of it which is new. The poets have known it for ages, it is in this their truth lies; and the philosophers, with poetical minds, the only true philosophers, have endeavoured to express it in words."

Seashore[1] also falls into line as soon as the word marginal is mentioned. "One of the most debatable issues in psychology pertains to the nature of this receding, marginal consciousness and what lies beyond, usually spoken of as the subconscious or unconscious. The idea of the subconscious has been a term to conjure with on the part of all kinds of mystery mongers in alleged psychology. For this there are several reasons. It is a field which has not yet been organized by systematic experiments in the laboratory, and, therefore, its full significance has not been grasped and it has not been adequately formulated in scientific psychology. It is not readily approached through direct introspection. It manifests itself most conspicuously in abnormal and pathological cases. It is by its very nature mysterious in that it often seems to function as a consciousness outside of the personal consciousness. In legitimate psychology it has furnished a most satisfactory explanation of the strange, the weird, and the apparently supernormal, as in hypnosis, alternating personality, and all forms of automatisms and is, therefore, resorted to by the ignorant and uncritical as a cover for anything which is mysterious to them. This, in turn, has made it an easy victim for identification with all sorts of uncritical, semi-religious, pseudo-philosophical and quasi-scientific theories as to its ultimate nature."

"There is a great diversity of opinion among psychologists on this subject, but, throughout this book, we are proceeding on the theory that the subconscious represents those processes which are outside of the focus of our attention to processes as related to the waking self. We regard the subconscious as an extension of consciousness, the distinction between conscious and subconscious being merely that we call those mental processes 'conscious' which we are aware of experiencing at the time of the experience in the waking state, and all other mental phenomena subconscious. As in the illustration of the landscape above, there is no sharp line between the focal region in which distinct features or objects are observed in turn and the vast region beyond this in which features are massed.

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[1] "Introduction to Psychology," 192, 194, 375 (1923).

"The term 'subconscious' seems to involve a contradiction. There is a tendency to identify conscious and mental; the subconscious would, therefore, imply something under the mental, not mental. This difficulty, however, rests mainly upon a poverty in words. According to our view, the subconscious is mental and it is the same kind as the conscious, the mental being divided into the conscious and the subconscious. It is merely remote in a part of the waking system of self-awareness. The subconscious may be clear, discriminating, and organized around a remote personal nucleus, just as the conscious is. In the analogy of the painting, one's observation may center about any feature, however trifling, to such an extent as to result in utter obliviousness to both self and painting as such. Our theory denies the assumption that ideas and impressions are stored, as implied in one group of theories; it disavows any metaphysical assumption in regard to the subliminal as a form of ultimate reality; it takes a broader view than the theory which regards the subconscious as a split-off fragment; it recognizes the adequacy of the physiological account of all neural processes underlying mentality; it recognizes the vastness of the subconscious element associated with our ordinary conscious life; it draws no sharp lines between the conscious and the subconscious; and does not attempt to determine the limit of mentality.

"The dream is a perfect example of the subconscious mental process, in that it is dissociated from the working consciousness of self to the extent that the dreamer is not aware of himself as dreaming and cannot control his dreams."

Jastrow is really campaigning against the subconscious or subliminal self and his book[1] contains many helpful things, though he apparently does not see the bearing of them as I do.

"Unusual activities of the subconscious will, in the main, occur only in unusual mental constitutions; normally, the emergence of a fairly independent piece of subconscious functioning depends upon a moderate variation from the standard illumination of conscious attention,—just the darkening of a passing cloud; abnormally, in favorable cases, the measure of its independence is decidedly emphasized and its more notable and impressive performances made possible. Yet throughout, the phenomena present consistent relations; the several factors that determine the result vary constantly and puzzlingly, and none more so than the individual temperament, the dominant integer[2] in the personal equation."

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[1] Jastrow: "The Subconscious," 62, 68, 95, 99, 159, 449 (1906).

[2] In this respect the mental states of children are interesting. Children enter into their occupations with a decided intentness and an emotional vivacity that bury them deep in the reality of their play; and they are likewise free from any considerable range of acknowledged claims to their attention. They accordingly furnish favorable opportunities for subconscious activity. At the same time the relative feebleness of their attention and the weakness of their habits militate against any very promising field for the exhibition of such subconscious traits. In actual observation the effect of both these tendencies may be readily observed. Children, if undisturbed, become absorbed in play and are most oblivious of what is going on about them, giving themselves singly and intensely to their play-fancies, forgetting their troubles, and occasionally falling into amusing lapses that exhibit the subconscious activities in formation. On the other hand, they weary quickly, require constant change of occupation, and welcome distraction if it is offered.

Jastrow quotes with approval the statement by Galton that "there seems to be a presence-chamber in my mind where full consciousness holds court, and where two or three ideas are at the same time in audience, and an antechamber full of more or less allied ideas, which is situated just beyond the full ken of consciousness. Out of this antechamber the ideas most nearly allied to those in the presence-chamber appear to be summoned in a mechanically logical way, and to have their turn of audience. . . . The exclusion of alien ideas is accompanied by a sense of effort. . . . The character of this effort seems to me chiefly to lie in bringing the contents of the antechamber more nearly within the ken of consciousness which then takes more comprehensive note of all its contents, and compels the logical faculty to test them *seriatim* before selecting the fittest for a summons to the presence-chamber." And further: "The thronging of the antechamber is, I am convinced, altogether beyond my control; if the ideas do not appear, I cannot create them, nor compel them to come.

"All this points to the fact that the large stores of accumulated learning which we carry in our heads lie in part near the focus of interest that occupies our immediate attention, in greater part lie in ever widening areas—all permeated by an intricate network of highways and byways along which the goods of our mind come floating. What Mr. Leland remarks of the work of genius is measurably true for the favored periods of all workers, namely, that 'it sweeps along, as it were, in a current, albeit it has enough reason left to also use the rudder and oars, or spread and manage a sail'; and though it is obvious that we cannot create the wind that brings the ship to port, we can guide the rudder and show our skill in using what breeze may come. Such a conception does not deceive itself that it explains what in fact it only describes; but it places the emphasis at the proper point, and avoids error by assimilating the unusual to the usual; it prevents the cherishing of false theories by shunning the assumption of marvels, and by extending the marvel of the commonplace. In every step of thought there is the unaccountable something, the hidden and individual motive power that supplies the energy; 'in the case of small steps, even the heavy and clumsy thinker feels sure that he does not trip; with greater leaps, however, the danger of stumbling increases, and only the dexterous and nimble attempt them with advantage.'[1] Though in time the flights may seem longer and more daring, and the contact with the earthly realities of consciousness may become so occasional and incidental as to create the feelings not of steps at all but of mysterious flight through the air, we may be assured that the feeling has no other than a subjective basis. The enthusiastic prophet of the Unconscious may tell us that 'the discursive and deductive method is only the lame walking on stilts of conscious logic, whilst rational intuition is the Pegasus flight of the Unconscious, which carries in a moment from earth to heaven'; yet the psychological observer cannot fail to notice the long periods of training and accumulation of experience that prepare the way for the marvelous performances of the expert. Admittedly in the end, the individual endowment remains the unaccountable factor in the problem; and most of us would make a poor showing with the seven-leagued boots of genius, were they suddenly to be placed at our disposal."

"There exists in all intellectual endeavor a period of incubation, a process in great part subconscious, a slow, concealed maturing through absorption of suitable pabulum. Schopenhauer calls it 'unconscious rumination,' a chewing over and over again of the cud of thought preparatory to its assimilation with our mental tissue; another speaks of it as the red glow that precedes the white heat. The thesis implied by such terms has two aspects: first, that the process of assimilation may take place with suppressed consciousness; second, that the larger part of the influences that in the end determine our mental growth may be effective without direct exposure to the searching light of conscious life. Both principles enforce the view that we develop by living in an atmosphere congenial to the occupation that we seek to make our own; by steeping ourselves in the details of the business that is to be our specialty, until the judgment is trained, the assimilation sensitized, the perspective of importance for the special purpose well established, the keenness for useful improvisation brought to an edge. When asked how he came to discover the law of gravitation, Newton is reported to have answered, 'By thinking about it.'

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[1] Hartmann.

"The scientific spirit—the most finished expression of conscious activity—finds its saving balance in the impressible imagination—the richest quarry of the subconscious. In all character, as in all achievement, there are talents more efficient than those consciously exercised, powers deeper than those we wittingly command, that enable us to do better than we know how. This recognition has ever been present in the conception of genius, picturing its incomprehensibility as an unquestioning response to an inspiration, as a surrender to the natural forces that seethe within, though reënforced by experience; as in its labors indifferent to means, oblivious to the why and wherefore, but firmly possessed with the imperative importance of its message, and leaving lowlier tasks to lesser minds that are constrained with painful deliberation to marshal in simple order the limited resources at their command. In this appraisement of the constituents of character, of the service of ideals in shaping culture, as of the quality of talents that further mental achievement, do the traditional wisdom of the ages and the analyses of psychology find common issue."

"Conscious utilization of subconsciously elaborated data remains the normal formula of thought. We label the product conscious when the drafts upon the reservoir are frequent and overt; we call them subconscious when fewer and elusive; but we hold the term peculiarly pertinent when the issue conforms more to purpose and interest than to mere capricious revery."

Dr. Morton Prince's experience with multiple personalities lead him to emphasize the phenomenon of coconsciousness; but except for that, his point of view[1] is distinctly less like that of the psychologist and more like that of other scientific men.

"In what I have said thus far I have had another purpose in view than that of a mere exposition of the psycho-physiological theory of memory. This other and chief purpose has been to lay the foundation for a conception of the *Unconscious* in its larger aspect. We have seen that thoughts and other conscious experiences that have passed out of mind may be and to an enormous extent are conserved and, from this point of view, may be properly regarded as simply *dormant*. Further we have seen that all the data collected by experimental pathology and other observations lead to the conclusion that conservation is effected in the form of neurographic residua or brain neurograms—organized physiological records of passing mental experiences of all sorts and kinds. We have seen that these neurographic records conserve not only our educational acquisitions and general stock of knowledge—all those experiences which we remember—but a vast number of others which we cannot spontaneously recall, including, it may be, many which date back to early childhood, and many which we have deliberately repressed, put out of mind and intentionally forgotten. We have also seen that it is not only these mental experiences which occupied the focus of our attention that leave their counterpart in neurograms, but those as well of which we are only partially aware—absent-minded thoughts and acts and sensations and perceptions which never entered our awareness at all—subconscious or coconscious ideas as they are called. Finally, we have seen that the mental experiences of every state, normal, artificial, or pathological, whatever may be the state of the personal consciousness, are subject to the same principle of conservation. In this way, in the course of any one's natural life, an enormous field of neurograms is formed representing ideas which far transcend in multitude and variety those of the personal consciousness at any given moment and all moments, and which are far beyond the voluntary beck and call of the personal consciousness of the individual.

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[1] Prince: "The Unconscious," 147, 150, 156, 159, 171, 212, 226, 245 (1921).

"Neurograms are concepts and, by the meaning of the concept, they are unconscious. It is not necessary to enter into the question whether they are in their ultimate nature psychical or physical. That is a philosophical question. They are at any rate unconscious in this sense; they are devoid of consciousness, i.e., have none of the psychological attributes of any of the elements of consciousness, and in the sense in which any physiological arrangement or process is not conscious, i.e., is unconscious. We have here, then, in the concept of brain residual neurograms the fundamental meaning of the *Unconscious*. *The unconscious is the great storehouse of neurograms which are the physiological records of our* *mental lives.* By the terms of the concept neurograms are primarily passive—the potential form, as it were, in which psychical energy is stored. This is not to say, however, that, from moment to moment, certain ones out of the great mass may not become active processes. On the contrary, according to the theory of memory, when certain complexes of neurograms are stimulated they take on activity and function—the potential energy becomes converted into dynamic energy. In correlation with the functioning of such neurographic complexes, the complexes of ideas which they conserve—the psychological equivalents—are reproduced (according to the doctrines of monism and parallelism) and enter the stream of the personal consciousness. The unconscious becomes the conscious (monism), or provided with correlated conscious accompaniments (parallelism), and we may speak of the ideas arising out of the unconscious.

"Here two important questions present themselves. It is a necessary consequence that when unconscious neurograms become active processes psychological equivalents must be awakened; and when they are awakened, must they *necessarily* enter the stream of the personal consciousness? If both these questions may be answered in the negative, *then plainly in either case such active processes become by definition subconscious processes*—of an unconscious nature in the one case and of a coconscious nature in the other. They would be subconscious because in the first place they would occur outside of consciousness and there is no awareness of them, and in the second place they would be a dissociated second train of processes distinct from those engaged in the conscious stream of the moment. Theoretically such subconscious processes, whether unconscious or coconscious, might perform a variety of functions according to the specificity of their activities.

"Now, in preceding lectures, when marshalling the evidence for conservation, we met with a large number and variety of phenomena (automatic writing, hallucinations, post-hypnotic phenomena, dreams, 'unconscious' solution of problems, etc.), which clearly demonstrated that memory might be manifested by processes of which the individual was unaware and which were outside the content of consciousness. Hence these phenomena presented very clear evidence of the occurrence of processes that may be properly termed subconscious. Attention, however, was primarily directed to them only so far as they offered evidence of conservation and of the mode by which conservation was effected. But necessarily these evidences were subconscious manifestations of forgotten experiences (memory), and in so far as this was the case we saw that unconscious neurograms can take on activity and function subconsciously; i.e., without their psychological equivalents (i.e., correlated conscious memory) entering the stream of the personal consciousness. We may now speak of these processes as subconscious memory. But when their manifestations are carefully scrutinized they will be found to exhibit more than memory. They may, for instance, exhibit logical elaboration of the original experiences, and what corresponds to fabrication, reasoning, volition, and affectivity. *Theoretically* this is what we should expect if any of the conserved residual experiences of life can function subconsciously. As life's experiences include fears, doubts, scruples, wishes, affections, resentments, and numerous other affective states, innate dispositions, and instincts, the subconscious memory process necessarily may include any of these affective complexes of ideas and tendencies. An affective complex means an idea (or ideas) linked to one or more emotions and feelings. In other words, any acquired residua drawn from the general storehouse of life's experiences may be systematized with feelings and emotions, the innate dispositions and instincts of the organism. Now it is a general psychological law that such affective states tend by the force of their conative impulses to carry the specific ideas with which they are systematized to fulfillment through mental and bodily behavior. Consequently, theoretically, it might thus well be that the residua of diverse experiences, say a fear or a wish, by the force of such impulses might become activated into very specific subconscious processes with very specific tendencies expressing themselves in very specific ways, producing very specific and diverse phenomena. Thus memory would be but one of the manifestations of subconscious processes.

"Now, as a matter of fact, there are a large number of phenomena which not only justify the postulation of subconscious processes but also the inference that such processes, activated by their affective impulses, may so influence conscious thought that the latter is modified in various ways; that it may be determined in this or that direction, inhibited, interrupted, distorted, made insistent, and given pathological traits. There is also a large variety of bodily phenomena which can be explicitly shown to be due to subconscious processes, and many which are only explicable by such a mechanism. Indeed, a subconscious process may become very complex and constellated with any one or many of the psychological mechanisms of the organism. In special artificial and pathological conditions where such processes reach their highest development, as manifested through their phenomena, they may exhibit that which when consciously performed is understood to be *intelligence,* comprising reasoning, constructive imagination, volition, and feeling; in short, what is commonly called thought or mental processes. Memory, of course, enters as an intrinsic element in these manifestations just as it is an intrinsic element in all thought. The automatic script that describes the memories of a long-forgotten childhood experience may at the same time reason, indulge in jests, rhyme, express cognition and understanding of questions—indeed (if put to the test), might not only pass a Binet-Simon examination for intelligence, but take a high rank in a Civil Service examination. In these more elaborate exhibitions of subconscious intelligence it is obvious that there is an exuberant efflorescence of the residua deposited in many unconscious fields by life's experiences and synthesized into a *subconscious functioning system*."

"*A subconscious process may be provisionally defined as one of which the personality is unaware, which, therefore, is outside the personal consciousness, and which is a factor in the determination of conscious and bodily phenomena, or produces effects analogous to those which might be directly or indirectly induced by consciousness.* It would be out of the question at this time to enter into an exposition of the larger subject—the multiform phenomena of the subconscious, but as its processes are fundamental to an understanding of many phenomena with which we shall have to deal, we should have a clear understanding of the grounds on which such processes are postulated as specific, concrete occurrences. The classical demonstration of subconscious occurrences makes use of certain phenomena of hysteria, particularly those of subconscious personalities and artificial 'automatic' phenomena like automatic writing. The epoch-making researches of Janet[1] on hysterics and almost coincidentally with him of Edmund Gurney on hypnotics very clearly established the fact that these phenomena are the manifestations of *dissociated* processes outside of and independent of the personal consciousness. Among the phenomena, for example, are motor activities of various kinds such as ordinarily are or may be induced by conscious intelligence. As the individual, owing to anesthesia, may be entirely unaware even that he had performed any such act, the process that performed it must be one that is subconscious."

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[1] Pierre Janet: "L'automatisme psychologique" (1889), and numerous other works.

"A subconscious personality is a condition where complexes of subconscious processes have been constellated into a personal system, manifesting a secondary system of self-consciousness endowed with volition, intelligence, etc. Such a subconscious personality is capable of communicating with the experimenter and describing its own mental processes. It can, after repression of the primary personality, become the sole personality for the time being, and then remember its previous subconscious life, as we all remember our past conscious life, and can give full and explicit information regarding the nature of the subconscious process. By making use of the testimony of a subconscious personality and its various manifestations, we can not only establish the actuality of subconscious processes and their intrinsic nature in these conditions, but by prearrangement with this personality predetermine any particular process we desire and study the modes in which it influences conscious thought and conduct. For instance, we can prescribe a conflict between the subconsciousness and the personal consciousness, between a subconscious wish and a conscious wish, or volition, and observe the resultant mental and physical behavior, which may be inhibition of thought, hallucinations, amnesia, motor phenomena, etc. The possibilities and limitations of subconscious influences can in this way be experimentally studied.

Subconscious personalities, therefore, afford a valuable means[1] for studying the mechanism of the mind."

"One of these two subjects, while in hypnosis and able to recollect what goes on in the secondary consciousness, thus describes the coconscious process during the *spontaneous* subconscious solution of problems. When a problem on which my waking self is engaged remains unsettled, it is still kept in mind by the secondary consciousness even though put aside by my waking self. My secondary consciousness often helps me to solve problems which my waking consciousness has found difficulty in doing. But it is not my secondary consciousness that accomplishes the final solution itself, but it helps in the following way: Suppose, for instance, I am trying to translate a difficult passage in Virgil. I work at it for some time and am puzzled. Finally, unable to do it, I put it aside, leaving it unsolved. I decide that it is not worth bothering about and so put it out of my mind. But it is a mistake to say you put it *out* of your mind. What you do is, you put it *into* your mind; that is to say you don't put it out of your mind if the problem remains unsolved and unsettled. By putting it *into* your mind I mean that, although the waking consciousness may have put it aside, the problem still remains in the secondary consciousness. In the example I used, the memory of the passage from Virgil would be retained persistently by my secondary consciousness. Then from time to time a whole lot of fragmentary memories and thoughts connected with the passage would arise in this consciousness. Some of these thoughts, perhaps, would be memories of the rules of grammar, or different meanings of words in the passage, in fact, anything I had read, or thought, or experienced in connection with the problem. These would not be logical, connected thoughts, and they would not solve the problem. My secondary consciousness does not actually do this, i.e., in the example taken, translate the passage. The translation is not effected here. But later when my waking consciousness thinks of the problem again, these fragmentary thoughts of my secondary consciousness arise in my mind, and with this information I complete the translation. The actual translation is put together by my waking consciousness. I am not conscious of the fact that these fragments of knowledge existed previously in my secondary consciousness. I do not remember a problem ever to have been solved by the secondary consciousness. It is always solved by the waking self, although the material for solving it may come from the secondary. When my waking consciousness solves it in this way, the solution seems to come in a miraculous sort of way, sometimes as if it came to me from somewhere else than my own mind. I have sometimes thought, in consequence, that I have solved it in my sleep.

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[1] The value of subconscious personalities for this purpose has been overlooked, owing, I suppose, to such conditions being unusual and bizarre, and the assumption that they have little in common with ordinary subconscious processes. But it ought to be obvious that *in principle* it makes little difference whether a subconscious system is constellated into a large self-conscious system called a personality, or whether it is restricted to a system limited to a few particular coconscious ideas. In the former case the possibilities of its interfering with the personal consciousness may be more extended and more influential, that is all.

"A series of observations conducted with a fourth subject (O. N.) gave the following results, briefly summarized. (This subject, like the others, is practised in introspection and can differentiate her memories with precision.) She distinguishes 'two strata' in her mental processes (an upper and lower). The 'upper stratum' consists of the thoughts in the focus of attention. The lower (also called the background of her mind) consists of the perceptions and thoughts which are not in the focus. This stratum, of course, corresponds with what is commonly recognized as the fringe of consciousness, and, as is usual, when her attention is directed elsewhere she is not aware of it. She can, however, bring this fringe within the field of attention and then she becomes aware of, or rather remembers, its content during the preceding moment. To be able to do this is nothing out of the ordinary, but what is unusual is this: by a trick of abstraction which she has long practised, she can bring the memory of the fringe or stratum into the full light of awareness and then it is discovered that it has been exceedingly rich in thoughts, far richer than ordinary attention would show and a fringe is supposed to be. It is indeed a veritable coconsciousness in which there goes on a secondary stream of thoughts often of an entirely different character and with different affects from those of the upper stratum. It is common for thoughts which she *has resolutely put out of her mind as intolerable or unacceptable, or problems which have not been solved, to continue functioning in the lower stratum without entering awareness.* She can, however, at any time become aware of them by the trick of abstraction referred to, and sometimes they emerge apparently spontaneously and suddenly replace the 'upper stratum.' In hypnosis also the content of the lower stratum can be distinctly recalled.

"Now the point I have been coming to is, the subject has acquired the habit of postponing the decision of many everyday problems and giving them, as a matter of convenience, to this second stratum or fringe to solve. She puts one aside, that is, out of (or *into*) her mind and it goes into this stratum. Then, later, when the time for action comes, she voluntarily goes into abstraction, becomes aware of the subconscious thoughts of the second stratum and, lo and behold! the problem is found to be solved. If a plan of action, all the details are found arranged as if planned 'consciously.' If asked a moment before what plans had been decided upon and decision reached she would have been obliged in her conscious ignorance to reply, 'I don't know.'

"An analysis of these different observations shows, first, that the post-hypnotic phenomena—calculations (a) and actions (b)—were performed by a subconscious process. Of this there can be no manner of doubt, even if the subsequent hypnotic memories of the process be rejected as untrustworthy. The phenomenon—the answer to the mathematical problem in the one case and the motor acts in the other—is so logically related to the suggestion, and can be predicted with such certainty, that only a causal relation can be admitted.

"Second, in the calculation phenomena the process is clearly of an intellectual character requiring *reasoning* and the coöperation of mathematical *memory*. (Reasoning is more conspicuous when the problem is more complicated, as in the calculation of the number of seconds intervening between say twenty-two minutes past eleven and seventeen minutes past three o'clock.) The phenomenon is the solution of a problem."

"I have suggested that the *subconscious intelligence* may be *comparable to the phenomenon of a coconscious personality*. It is worth noting in this connection that in the case of Miss B. the coconscious personality, Sally, who claimed to be awake while Miss B. was dreaming, also claimed that Miss B. sometimes dreamed about what Sally was thinking of at the moment. In other words, the thoughts of a large systematized coconscious intelligence determined the dream just as these thoughts sometimes emerged into Miss B.'s mind when awake. That a coconscious personality may persist awake while the principal personality is asleep I have been able to demonstrate in another case (B. C. A.). It was also noted in Dr. Barrows' case of Anna Winsor. Moreover, Sally was shown to be a persistent sane coconsciousness while Miss B. was delirious and also while she was apparently deeply etherized and unconscious. After all it is difficult to distinguish *in principle* the condition of sleep with a persisting coconsciousness from a state of deep hypnotic trance where the subject is apparently unconscious. In this condition, although the waking consciousness has disappeared, there can be shown to be a persisting 'secondary' consciousness which can be communicated with by automatic writing and which later can exhibit memories of occurrences in the environment during the hypnotic trance. (B. C. A.).

"What has been said does not touch, of course, the other mechanisms of the Freudian theory nor the unessential, greatly over-emphasized, theory that the subconscious dream is always a sexual wish. On the contrary, the principle throws a strong, *a priori* doubt upon the correctness of this generalization. It is plainly, however, a matter of fact which might be easily determined by observation were it not for the difficulty of correctly referring clinical phenomena to the correct antecedent experiences as their causal factors. In the last analysis it becomes always a matter of interpretation.

"Consider certain facts of every-day experiment. A novel and difficult question is put up to us for decision. We have, we will say, to decide whether a certain piece of property situated in a growing district of a city shall be sold or held for future development: or a political manager has to decide whether or not to pursue a certain policy to win an election; or the President of the United States has to decide the policy of the government in certain land questions in Alaska. Now each of us would probably say that we could not decide such a question offhand; we would want time for consideration.

If we attempted voluntarily, at the moment the question is put, to recall to mind all the different facts involved, to consider the given question from all aspects, to switch the main facts into their different settings, we would find it an impossible thing to do. We consequently take the matter 'under advisement,' to use the conventional expression. We want time. Now what we apparently, and I think undoubtedly, do is to put the problem *into* our minds and leave it, so to speak, to incubate. Then, from time to time, as we take up the matter for consideration, the various facts involved in the different aspects of the question, and belonging to their different settings, arise to mind. Then we weigh, compare, and estimate the value of these different facts and arrive at a judgment. All happens as if subconscious processes had been at work, as if the problem had been going through a subconscious incubation, switching in this and switching in that set of facts, and presenting them to consciousness, the final selection of the deciding point of view being left to the latter. The subconscious garners from the store-house of past experiences, those which have a bearing on the question and are required for its solution, brings them into consciousness, and then our logical conscious processes form the judgment. The degree to which subconscious processes in this way take part in forming judgments would vary according to the mental habits of the individual, the complexity of the problem, the affectivity and conflicting character of the elements involved. Under this theory we see that there is a deeper psychological basis for the every-day practice of taking 'under advisement' or 'into consideration' a matter, before giving judgment, than would appear on the surface. There is considerable experimental evidence in favor of this theory. In discussing above the subconscious solution of problems I cited certain evidence obtained from the memories of subjects in hypnosis, for coconscious and unconscious processes taking part in such solutions. I have been able to accumulate evidence of this kind showing the co-operation of processes outside of consciousness in determining the point of view and final judgment of the subject when a matter has been under advisement; particularly when the subject has been disturbed by doubts and scruples. It is plain that in the final analysis any question on which we reserve our judgment is a problem which we put *into* our minds. And, after all, *it is only a question of degree and affectivity between the state of mind which hesitates to decide an impersonal question, like a judicial decision, and one that involves a scruple of conscience.* This state often eventuates in hallucinatory and other phenomena involving subconscious processes. Scruples of conscience, it is true, usually have strong affective elements as constituents, but the former may also have them, particularly when involving personal ambitions, political principles, etc.

"If the subconscious processes which perform a mathematical calculation and other problems, which logically determine the symbolism of a dream, etc., can be correctly interpreted as unconscious, they plainly exhibited a higher order of intelligence than any conscious processes in lower animals, or even some conscious processes of man, like brushing away a fly."

I am glad to have this opportunity to line up with Dr. Holmes and others. In my own case most of the good ideas come as a kind of inspiration and the unconscious reasoning seems to me rather better than my conscious reasoning. If I could draw oftener on the supra-marginal consciousness, it would be distinctly helpful. Of course the ideas that come in from somewhere deal with problems with which I am also struggling consciously. It does not seem to me necessary to postulate a second personality or a greatly better mental technique. It might be enough that a wider range of facts was available at a given moment.

I have tried to show you that there is nothing uncommon about this, and that a great many scientific men feel the same way. I do not even know whether I am an extreme case or not. Since this feeling of a supra-marginal consciousness is so strong with many people, how does it happen that the psychologists ignore it so completely and so consistently? A possible explanation has occurred to me.

One day after the war I was talking to four chemists and I made some reference to the subliminal self, not knowing at that time that I should have said supra-marginal consciousness. To three of the men the statement was a mere common-place. The fourth did not understand what I meant and said so. It then appeared that he had never had the feeling of an inspiration, of an idea coming unexpectedly from somewhere to the region of consciousness. He had worked consciously for all his results and we were speaking a language which meant nothing to him. I have wondered, since then, whether psychologists as a class are men who either do no unconscious thinking or whose unconscious thinking never comes suddenly into the field of consciousness. That would explain all the facts; but I do not know whether it is the right explanation. The position taken by the psychologists is that the rest of us are deceiving ourselves. That may be true, but it does not appeal to me as being probable. One does not see why the minds of some psychologists should not work like those of other people; but apparently they do not. So far as I know, there is no urge among color-blind people to become physicists and it is queer that an analogous thing should apparently be characteristic of psychologists. In connection with this, one could very well use the quotation from John Buchan as a test. I think that that paragraph would strike a responsive chord in a great many people and that to others it would be quite meaningless. Would the majority of the psychologists come quite overwhelmingly in one of these classes? I have also wondered whether perhaps feminine intuition may not be due to the fact that the unconscious reasoning comes into consciousness more often with women than with men.

We know that there are many cases where actions take place without there being normal consciousness.[1] "Of conditions conforming to this status the most familiar is somnambulism. As a modification or accident of sleep, it presents an altered disposition of brain-functioning, whereby a part of the mental machinery is set into action without arousing the rest. The mental condition of the somnambulist is an interesting one, and not so much for what it leads him to do, as for his attitude and sensibilities while thus occupied. He is manifestly not wholly awake; his senses respond to a peculiarly circumscribed range of stimuli, and his actions make no report to that phase of consciousness upon which his waking memory depends. Unmistakable circumstantial proof falls short of completely convincing him that it was he who performed in sleep the versatile achievements that the normal memory so completely repudiates, for the very reason that the sleep-acting self is not the self—not the complete self—that conducts the introspective inquiry.

"The objective evidence is fortunately quite definite. There is in the older literature the record of a sleep-walker whose inquiring friends tested his powers while engaged in his nocturnal excursions. With a restricted type of awareness, he saw and felt and recognized familiar objects, and behaved toward them in routine, partly intelligent fashion.

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[1] Jastrow: "The Subconscious," 267 (1906).

If a pipe were placed in his hands, the somnambulist handled it correctly, but could not light it; if it were lighted for him, the pipe went out because he did not inhale properly. He could be induced to sit at a table and go through the actual movements of writing. If given a book, he turned its pages, resting his gaze on each page, but without reading; and he continued in this automatic mimicry if the light were withdrawn. When forcibly aroused, he was shocked to find himself out of bed and in the presence of his friends. Had he awakened of his own accord, he might, with equal suddenness, have come to himself and without memory of his immediate occupation.

"Observations of this general import have been sufficiently verified to establish that the somnambulist is suggestible; that to some extent an appeal to his senses arouses appropriate response; that he, in part, appreciates the felt and seen positions and nature of things; and that his reactions, though automatic, reflect a simply intelligent yet limited adaptation to routine situations. They show further that spontaneously he takes cognizance only of that particular area of sensations and movements that fits in with his self-imposed quest. The somnambulist, bent upon finding a lost object, avoids obstacles, manipulates latches and locks and keys and doors and drawers, finds the proper material in the kitchen for washing dishes or baking a pie, but is insensitive to the happenings about him, does not hear or see the person who, with lighted candle, is approaching to awaken him, and is likely to stumble against any familiar object, and unintelligently to fumble about the knob of the door that, without his knowledge, has been locked to prevent his escape. Thus thwarted in his purpose, he may wander back to bed without awakening; yet a sufficiently violent stimulus breaks through the narrow circle of his contracted perceptions, and brings him to normal wakefulness. Many a somnambulist who is aware of his failing, and who has found it unavailing to lock the door and to hide the key (the nocturnal consciousness being quite equal both to securing the key and to opening the door), has resorted to the expedient of dropping the key into a basin of cold water, relying upon the shock, when the sleeper's hands were plunged into the water, to awaken him. This group of possibilities and limitations of mental behavior sufficiently establishes the close affiliation of natural somnambulism to other conditions, and especially to hypnosis, and indicates that what they have distinctly in common is the general type of mental disintegration that permits the spontaneous or suggested episode to be enacted without sanction or knowledge of the normally directing stage-manager."

Seashore[1] says that "sleep-walking occurs only in deep sleep. Many apparently fabulous stories of feats in sleep-walking are found true. A college student formed the habit of getting up in sleep, dressing, walking down to the Mississippi River three-quarters of a mile distance, undressing, taking a deliberate and enjoyable swim, dressing, walking back to his room, undressing and retiring, only to wake up in the morning without the slightest inkling of remembrance from the escapade of the night. But when his friends constituted themselves detectives and awakened him suddenly in the act, the whole performance stood out clear to him in his memory. Sleep-walking is dream-action. If a sleep-walker is allowed to return to bed without being awakened, he will have no memory of the dream action in the morning."

I had an interesting experience myself in my freshman year. I was playing on the freshman eleven and we were practising with the varsity. The varsity was not a very good

[1] "Introduction to Psychology," 368 (1923).

team that year and on that day it was particularly bad. The men were missing all sorts of tackles. Finally the full-back, who was a good player, made up his mind that he would down the next man who got through to him and down him hard. For some unknown reason I was the next man and the full-back stood me on my head in a good, workman-like manner. I can remember people telling me that I had better stop playing, which did not appeal to me. I finally convinced people that I was not hurt and the game went on. The next thing that I can remember, I could both see and hear. We were lined up in the middle of the field; but the man in front of me was not the man who had been playing opposite me when I got hurt. He was the regular end who had not been able to get out on time. I got that mysterious thing straightened out without asking any questions. The next time I came to, I could hear but I could not see. I had the football in my hands, though I could not see it, and people were discussing some play. By listening I learned that I had made a fair catch; but that perhaps I had been offside. At that time the ball had to be kicked after a fair catch and I remember wondering how I was going to kick a ball I could not see. That difficulty never arose, because it was ruled that I was offside. The ball was taken away from me and my memories cease until the game was over and we started to walk off the field. I was then able both to see and to hear. We had to go across Holmes field to get to the gymnasium. I remember stepping over the low fence surrounding Jarvis field and the next thing I was going up the steps of Hemenway gymnasium. I had walked across Holmes field in a state of apparent unconsciousness without the people I was with noticing anything abnormal. In fact I was told that I had played better football that afternoon than ever before.

If one admits my account of what happened that afternoon, there must have been a great deal of mental and physical adjustment which was independent of normal consciousness, and the solving of problems in the supra-marginal consciousness may be another form of the same thing. The psychologist takes the ground that my account of what happened is grossly inaccurate. He believes that I could see and hear all the time; but that there were interesting lapses of memory. This may be true. It is well-known that after an accident there is very often a loss of memory as to events preceding the accident. It does not seem to me that the episode of the ball which I could feel but not see comes within that category. I believe that, as a result of shock, I was in a state somewhat analogous to somnambulism or hypnotism, and that things happened substantially as I have described them. The experiments of Dr. Morton Prince on disintegrated personality do not bear directly upon this; but they do illustrate the general principle that one can have thought and knowledge quite independent of normal consciousness.

Since we cannot at present control our unconscious thinking, it seems to be a matter of accident or fate whether a good idea comes into the field of consciousness. It is that, together with the belief in law, that makes scientific men believe more or less strongly in predestination quite apart from golf. That does not mean that we can sit down and wait, because the ideas do not come to those who do not work. It is on the knees of the gods whether there will be a loose ball in the football field in any particular game; but that accident will not mean a touchdown unless there has been a great deal of preliminary training. Mere natural alertness is not adequate.

When I first came back from Germany over forty-five years ago, I started some work on mass law relations in systems like benzene, alcohol and water, where there could be two liquid layers. I thought then and still think that it was the best work I was ever likely to do. Nobody else, not even my close personal friends, thought it was any good at all. That was quite natural. I had a fractional exponent for which I could not and cannot now account, and fractional exponents were taboo before the days of the adsorption isotherm. I also ran counter, without explanation, to many of the things that the physical chemists held most sacred in those days. People were interested in variations from the gas laws and from normal molecular weights. Alcohol in benzene was known to be thoroughly abnormal. It still is. It was not reasonable to suppose that addition of water, an associated liquid, could do anything but increase the abnormality of the results and yet benzene, alcohol and water fitted into a simple formula over the whole range of concentrations with an error not to exceed one-tenth of one per cent. The thing was impossible and the most charitable view was that I was deluding myself.

If I could have straightened the thing out at that time, people would probably have cited me as another case of genius manifesting itself early. As it is, I am not cited as a case of genius at all. I reached the point where I did not know what to do next, and we dropped the subject for a quarter of a century or more. Quite recently the clouds lifted and I saw what to do next. We are working on the problem again. Even now I do not see through to the end. If the gods are willing, we shall finish the matter up in a few years. If the fog closes down again, as it may, the problem will be left to some better man to solve.

It is because we realize how much of the results depend upon factors outside our control, that the scientific man does not, as a rule, have a swelled head. The night before we sailed for Galveston we heard Walter Hampden in King Henry V. After the curtain goes down, the English soldiers sing at night on the field of Agincourt. The scientific man might perhaps word it a little differently; but the sentiment would be the same in the two cases. The soldiers sang the 115th Psalm: "Non nobis, domine, non nobis, sed nomini tuo da gloriam."

WILDER D. BANCROFT.

R.R. DONNELLEY & SONS COMPANY · THE LAKESIDE PRESS · CHICAGO

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The Rice Institute Pamphlet is published at Houston, Texas, and is issued in January, April, July, and October. It was entered as second-class matter, April 15, 1915, at the post-office at Houston, Texas, under the Act of August 24, 1912